



HANDBOUND  
AT THE



UNIVERSITY OF TORONTO



Digitized by the Internet Archive  
in 2007 with funding from  
Microsoft Corporation









11825- (62) 9467  
T 814  
PSYCHOLOGICAL REVIEW PUBLICATIONS

THE  
Psychological Review

EDITED BY

JOHN B. WATSON, JOHNS HOPKINS UNIVERSITY

HOWARD C. WARREN, PRINCETON UNIVERSITY (*Index*)

JAMES R. ANGELL, UNIVERSITY OF CHICAGO (*Monographs*) AND

ARTHUR H. PIERCE, SMITH COLLEGE (*Bulletin*)

ADVISORY EDITORS

R. P. ANGIER, YALE UNIVERSITY; MARY W. CALKINS, WELLESLEY COLLEGE; RAYMOND DODGE, WESLEYAN UNIVERSITY; H. N. GARDINER, SMITH COLLEGE; JOSEPH JASTROW, UNIVERSITY OF WISCONSIN; C. H. JUDD, UNIVERSITY OF CHICAGO; ADOLF MEYER, JOHNS HOPKINS UNIVERSITY; HUGO MÜNSTERBERG, HARVARD UNIVERSITY; W. B. PILLSBURY, UNIVERSITY OF MICHIGAN; C. E. SEASHORE, UNIVERSITY OF IOWA; G. M. STRATTON, UNIVERSITY OF CALIFORNIA; E. L. THORNDIKE, COLUMBIA UNIVERSITY

VOLUME XIX., 1912.

129635-  
22/10/13  
PUBLISHED BI-MONTHLY BY

PSYCHOLOGICAL REVIEW COMPANY

41 NORTH QUEEN ST., LANCASTER, PA.

AND PRINCETON, N. J.

AGENTS: G. E. STECHERT & CO., LONDON (2 Star Yard, Carey St., W. C.);  
LEIPZIG (Hospital St., 10); PARIS (76 rue de Rennes);

Entered as second-class matter July 13, 1897, at the post-office at Lancaster, Pa., under  
Act of Congress of March 3, 1879.

BF  
1  
27  
19



PRESS OF  
THE NEW ERA PRINTING COMPANY  
LANCASTER, PA.



III

## CONTENTS OF VOLUME XIX.

### *January.*

- The Retina and Righthandedness. H. C. STEVENS AND C. J. DUCASSE, 1.  
Difference-Sensibility for Rate of Discrete Impressions. KNIGHT DUNLAP, 32.  
Some Novel Experiences. H. A. CARR, 59.  
The Influence of Caffein on the Speed and Quality of Performance in Typewriting.  
H. L. HOLLINGWORTH, 65.  
A New Memory Apparatus. F. KUHLMANN, 73.

### *March.*

- The Relation Between Mode of Presentation and Retention. V. A. C. HENMON, 79.  
Combining the Results of Several Tests: A Study in Statistical Method.  
R. S. WOODWORTH, 97.  
Knowing Selves. JOHN E. BOODIN, 124.

### *Discussion:*

- Professor Titchener's Theory of Memory and Imagination. A. E. DAVIES, 147.  
Memory and Imagination: A Restatement. E. B. TITCHENER, 158.

### *May.*

- The Curve of Work. EDWARD L. THORNDIKE, 165.  
Colored After-Image and Contrast Sensations from Stimuli in which no Color is  
Sensed. C. E. FERREE AND GERTRUDE RAND, 195.  
A New Laboratory Pendulum. KNIGHT DUNLAP, 240.  
Discussion:

Can Biology and Physiology Dispense with Consciousness.

ELLIOT P. FROST, 246.

### *July.*

- The Question of Association Types. FREDERICK LYMAN WELLS, 253.  
Experimental Studies in Rhythm and Time. J. E. WALLACE WALLIN, 271.  
III. The Estimation of the Mid-Rate between two Tempos.  
Literary Self Protection. JUNE E. DOWNEY, 299.  
A Time Experiment in Psychophysics. D. O. LYON AND H. L. ENO, 312.

### *September.*

- Introspective Analysis of Certain Tactual Phenomena. GEORGE F. ARPS, 337.  
Esthetics of Simple Color Arrangements. KATE GORDON, 352.  
An Optics-Room and a Method of Standardizing its Illumination.  
C. E. FERREE AND GERTRUDE RAND, 364.  
The Sensation of Movement. JOHN E. WINTER, 374.  
Mind as Middle Term. ROBERT MACDOUGALL, 386.  
Discussion: The Case Against Introspection. KNIGHT DUNLAP, 404.

### *November.*

- The Nature of Perceived Relations. KNIGHT DUNLAP, 415.  
The Effect of Length of Series upon Recognition Memory. EDWARD K. STRONG, 447.  
The Effect of Changes in the General Illumination of the Retina upon its Sensitivity  
to Color. GERTRUDE RAND, 463.  
Note on a Retrial of Professor James' Experiment on Memory.  
HARVEY A. PETERSON, 491.





# THE PSYCHOLOGICAL REVIEW

---

## THE RETINA AND RIGHTHANDEDNESS<sup>1</sup>

BY H. C. STEVENS AND C. J. DUCASSE

### I. (a) INTRODUCTION

The motives which have actuated experimental investigations into the capacity of the retina to estimate spatial extents have been various. The older observers, Fechner,<sup>2</sup> Volkman,<sup>3</sup> Chodin<sup>4</sup> and Fischer<sup>5</sup> were concerned mainly to prove or disprove the applicability of Weber's law to extensive magnitudes. Kundt<sup>6</sup> and Münsterberg<sup>7</sup> seem to have been interested chiefly in the light that constant errors in retinal space perception might throw upon the rôle of the eye muscles in judgments of extent. In a third class belongs the work of Highier<sup>8</sup> and Merkel<sup>9</sup> who carried out a series of experiments upon the estimation of retinal magnitudes for the purpose of furnishing numerical results for testing the validity of the psychophysical methods. The present work was also undertaken with a special end in view. In papers previously published<sup>10</sup> one of the authors has attempted to show that very essential differences, in space sense, exist between symmetrical positions upon non-corresponding halves of the two retinas. The purpose of the present paper is to demonstrate in a differ-

<sup>1</sup> From the Psychological Laboratory of the University of Washington.

<sup>2</sup> 'Elemente der Psychophysik,' II., 343.

<sup>3</sup> 'Physiologische Untersuchungen im Gebiete der Optik,' 1863.

<sup>4</sup> *Archiv f. Ophth.*, Bd. 23, S. 92.

<sup>5</sup> *Arch. f. Ophth.*, Bd. 37, S. 109.

<sup>6</sup> *Pogg. Annalen der Physik*, Bd. 120, S. 118.

<sup>7</sup> *Beiträge zur Exper. Psych.*, Heft 2, S. 125.

<sup>8</sup> *Phil. Studien*, Bd. 7, S. 232.

<sup>9</sup> *Phil. Stud.*, Bd. 9, SS. 53; 176; 400.

<sup>10</sup> *Psych. Rev.*, 15, 69; 15, 373.

ent way and, perhaps, with greater rigor of method and technique the same fact anew.

For a statement of the results of the experiments we cannot do better than quote from a summary<sup>1</sup> already published. "Experiments<sup>2</sup> carried out during the past year, on the comparative sizes of objects which are seen in indirect vision, brought to light the fact that a marked difference in the perception of size exists between the right and left halves of the retinae of the two eyes. The experiments were made with a perimeter.<sup>3</sup> The objects compared were the orbits described by two black spots which were borne upon the peripheries of two slowly moving white cardboard discs. The spots were attached to movable radii so that the orbit of the apparently larger disc could be reduced until it equaled, subjectively, the orbit of the smaller. In this way, quantitative measurements were made for four meridians, vertical, horizontal and two oblique (each inclined 45 degrees to the right and left of the vertical respectively) and for three parallels of latitude, 10°, 20° and 25° of the visual field. The observations were either (a) *peripheral comparisons* in which the discs were situated in the periphery of the field of vision upon some one of the four meridians upon opposite sides of (and at equal distances from) the fixation point; or (b) *foveal-peripheral comparisons*, in which one disc covered the fixation point and the other occupied some position in the periphery. The results of both (a) and (b) follow. (i) The discs upon the upper vertical, right-upper oblique, right horizontal and right-lower oblique meridians appear larger than similar discs symmetrically placed at opposite sides of the fixation point or at the fixation point. (ii) This result is constant for *both* eyes. (iii) The enlargement is greatest at 25° from the fixation point and least at 10°. (iv) The enlargement is greater in the right upper field than in the right lower field." Figures 1, 2, 3, and 4 of the article<sup>3</sup> already referred to show in graphic form each of the propositions just stated.

<sup>1</sup> *Science*, N. S., XXVII., 272.

<sup>2</sup> *Psych. Review*, XV., 69.

<sup>3</sup> For particulars concerning the apparatus, see the article in the *Psych. Rev.*, XV., 69.



(b) DESCRIPTION OF THE APPARATUS

The successful attack of a psychophysical problem requires an adequate form of apparatus. This truth was brought home to us only after making several hundred observations with a crude instrument which we were forced eventually to abandon. This instrument consisted of a meter stick, one side of which had been planed smooth and blackened, which was bolted through its middle point to a vertical standard about one meter in height. Attached to the stand-



PLATE I.



PLATE II.

ard was a protractor, from which the angular position of the meter stick which was capable of rotation about the bolt which attached it to the standard, could be determined. The head of the bolt served as the fixation point; the outer limits of the standard and variable extents were marked by two white threads held crosswise to the meter stick by two metal frames which could be slipped along the stick or fixed at chosen points. The observer sat with his head supported in a frame to secure uniformity of position, with one eye upon the fixation point while the experimenter moved to a point of subjective equality with the standard extent, the thread

limiting the variable extent. Observations with three subjects with standard extents of 40 and 80 mm. yielded mean variations too large to be of use in reaching a solution of our problem. We were therefore compelled to design a more exact form of instrument.

Plates I. and II. give a general idea of the apparatus as a whole. A detailed description of the parts follows.

The diameter of the disc is 61 centimeters. It is made of galvanized iron, stiffened at the back as shown in the Plate II. by flat strips of metal  $25 \times 4$  mm. riveted on. Black velvet is stretched on the front and held in place by sewing all round through holes drilled in the edge of the disc.

Figure 1, Plate III., shows a round plate *A*, 110 mm. in diameter, riveted to the center of the disc at the back. To this plate is screwed the sleeve *B* into which the hollow axis *C* (shown in dotted lines) fits and is secured by a set-screw, *K*. The large gear *E*, grooved wheel *F* (which receives the belt connecting with the cranks) and collar *G*, are in one piece, which revolves freely upon the axis *C*. *H* is a separate free collar between the face of the top plate *I* and the revolving piece *EFG*. *J* is a collar secured by set-screws to the end of the axis, to prevent it from sliding out of its tunnel in the top plate *I*. *K* is the thumb screw which secures the axis, and therefore the whole disc, in any desired position. *L* is a small blackened metal button on the face of the disc. It bears the center sight (fixation point) which is of silver and  $2\frac{1}{2}$  mm. in diameter. On each side of the sight, a small aperture is drilled through the button, opening into the drill hole which runs through the length of the axis *C*. This permits of using the apparatus not only for the comparison of empty intervals, but of filled magnitudes as well. To do this, two slender white cords are attached to the under surfaces of the flat sight-bearing pieces *M* by knotting the ends and slipping them into the slots shown in *M*<sub>2</sub>, Fig. 4. The free ends of the cords are then passed through the apertures in the button, then through the hollow axis, and a lead weight attached to each to stretch them taut (Fig. 1*a*).

The remaining features of Fig. 1 require little explanation.



The arm *P* is of blackened metal  $25 \times 4$  mm. and graduated in millimeters on the back as shown in Fig. 3. The collar *N*, Fig. 1, is attached to the rod *S* by a set screw and should be fitted closely enough against the face of the support *Q*, to prevent any lengthwise motion of the rod *S*, without, however, impeding its rotation. The thread on the rod *S* is exactly one millimeter, thus propelling the carriage *R* one millimeter to one turn. A small brass cylinder *O* bearing one hundred numbered divisions is secured to the rod *S* by a set-screw, and a pointer *T* is attached to the support *Q*<sub>2</sub>. The passage under the pointer of one division of the cylinder therefore corre-

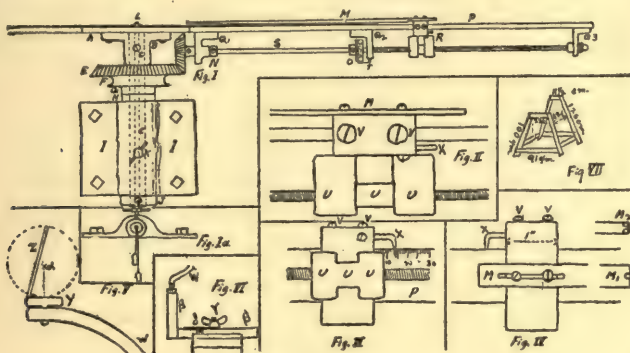


PLATE III.

sponds to a lateral displacement of the carriage (and connected sight) of one hundredth of a millimeter.

Different views of the carriage *R* are represented in Figs. 2, 3, and 4. The carriage itself is 25 mm. wide, but the threaded sleeve *U* into which the rod passes is 37 mm. long. This length was found necessary to prevent absolutely any looseness and to secure a positive displacement of the carriage to the slightest rotation of the rod *S*. The screws *VV* bear upon the upper edge of the arm *P* and permit of taking up any wobble of the carriage on the arm. *X* is the pointer for the millimeter scale. Fig. 4 shows the manner of attachment of the sight-bearing piece *M* to the face of the carriage. *M* represents the end of the sight-bearing piece with its silver sight  $2\frac{1}{2}$  mm. in diameter. *M*<sub>2</sub> is the obverse of *M*, and shows the end doubled under and slotted for receiving the

knotted end of the cord. The arm on the standard side of the disc is also graduated in millimeters. Its carriage and sight-bearer are similar to those on the variable side, except that no sleeve *U* is attached to the carriage, a thumb-screw being substituted to clamp it on the arm at any desired point.

Figures 5 and 6 represent the eye-piece and its support. The lower part of the curved rod *W* is threaded and fitted with a nut to permit of vertical adjustment. The wooden piece *BB* is slotted in the center and secured in any position desired by the winged nut *Y*. It is fitted with a scale and pointer *S* indicating the distance between the point  $\alpha$  (Fig. 5) and the center sight fixation point on the disc. The upper end of the curved rod *W* is flattened and drilled, the table-shaped piece *Y* revolving freely, but without wobble, in the drill hole. To the piece *Y* is soldered the ring *Z* at such an angle that when the eye is applied to it and the line of sight directed to the center sight on the disc, that part of the anterior surface of the cornea which is in the line of sight, occupies the point  $\alpha$ . This point, which measures 525 mm. from the fixation point, is at the intersection of the vertical axis of the piece *Y* and of a line passing through the center of the center sight on the disc, and perpendicular to it.

The construction of the stand is shown in Plates I. and II. and Fig. 7. It is made of hardwood 15  $\times$  75 mm. The dimensions are indicated in Fig. 7. The machine can be operated either by the person who observes or by the person who records. A black velvet curtain with a circular opening 59 cm. in diameter should be hung directly in front of the disc. Subdued artificial light was used to insure equal illumination throughout the experiments.

### (c) CONDITIONS OF EXPERIMENTATION

The apparatus permits of observation under a great variety of conditions.

- i. With a solid white line for either the standard or variable extent or both.
- ii. With a black extent limited by two white spots for either standard or variable or both.

- iii. With comparisons in any meridian of the field of vision.
- iv. With adjustment made either by the experimenter or by the observer.
- vi. With standard or variable extent upon the same or opposite side of the fixation point.
- vii. With standard and variable extents separated by an interval or adjacent to one another.

In our own experiments the standard and variable were black extents limited by white spots; adjustments were made by the observer; standard and variable lay upon opposite sides of the fixation point; and the extents were adjacent to each other at the fixation point. All of the observations were made with one eye at a time. The eye with which the observation was made was always fixed upon the fixation point; during an observation it never moved. The non-observing eye was covered by a screen or held shut. Between observations the observing eye was turned towards the floor. Observations were always made with a regular alternation of right and left eyes after ten readings. Standard and variable extents were presented alternately on opposite sides of the fixation point.

After repeated attempts in different rooms of the laboratory, to find a uniform and equally distributed source of light, we were forced to have recourse to the dark room, where errors due to unequal illumination and reflections from walls and ceiling could be eliminated. With an illumination of about four candle power, the limiting white spots stood out against the black velvet background with clearness. To cut off reflected light which brought out the difference in blackness between the black of the velvet and that of the painted sight carriers, black curtains were suspended in front of the apparatus.

The method used in these experiments was that form of the method of average error which Müller<sup>1</sup> calls 'the determination of equivalent stimuli by means of the method of limits.' The constant error for the reproduction of each standard extent in each of the four meridians investigated

<sup>1</sup> 'Die Gesichtspunkte und die Thatsachen der psychophysischen Methodik,' p. 201.



was determined from forty settings of the variable extent. Of these settings, twenty were from greater to equal and twenty from less to equal. The readings were made by one of the authors from a position behind the apparatus and written upon a card printed for the purpose. The card, a specimen of which is here shown, records the meridian, the side upon which the standard lies, the eye used, the individual readings (Fehlreizen in Müller's terminology) in the order in which they were taken, the average of the readings, the constant error, the mean variation of the readings, and the probable error for the series.<sup>1</sup>

Standard: 40 mm.

Mean: 44.61.

Angle: 135°.

C.e. = 4.61.

O. Dr. Magnusson.

E. C. J. D.

Date, Nov. 18, 1908.

Time 11:30 A.M.

Eye: Left.

M.v. = 1.53.  $P_{em} = 0.208$ .

1. G. to E. 45.93
2. L. to E. 43.89
3. G. to E. 44.57
4. L. to E. 44.00
5. G. to E. 48.23
6. L. to E. 45.64
7. G. to E. 46.62
8. L. to E. 44.24
9. G. to E. 44.61
10. L. to E. 43.65
11. G. to E. 49.69
12. L. to E. 46.13
13. G. to E. 48.21
14. L. to E. 47.63
15. G. to E. 47.00
16. L. to E. 44.01
17. G. to E. 44.04
18. L. to E. 42.72
19. G. to E. 46.70
20. L. to E. 46.58
21. G. to E. 45.57
22. L. to E. 42.49
23. G. to E. 44.10
24. L. to E. 41.10

<sup>1</sup> The probable error of the series was computed from the approximate formula given by Titchener, 'Experimental Psychology,' Vol. II., Pt. I, p. 65.

$$P_{em} = \frac{0.85 MV}{\sqrt{n-1}}$$

25. G. to E.	45.55
26. L. to E.	43.18
27. G. to E.	42.95
28. L. to E.	42.80
29. G. to E.	44.30
30. L. to E.	42.10
31. G. to E.	44.10
32. L. to E.	43.20
33. G. to E.	44.30
34. L. to E.	42.20
35. G. to E.	42.30
36. L. to E.	42.25
37. G. to E.	44.25
38. L. to E.	45.80
39. G. to E.	44.77
40. L. to E.	<u>43.35</u>
	1784.75

The arithmetical mean of the twenty greater to equal (G. to E.) readings gives the  $F_0$  of Müller, or that extent which no longer appears greater than the standard which in this case is 45.38; the arithmetical mean of the twenty less to equal (L. to E.) readings gives the  $F_u$  of Müller, or that extent which no longer appears smaller than the standard, in this case 43.84. The difference of  $F_0$  from the standard gives the average G. to E. error,  $\Delta_0$  or 5.38; similarly, the difference of  $F_u$  from the standard gives the average L. to E. error,  $\Delta_u$  or 3.84; then  $\frac{\Delta_0 + \Delta_u}{2} = 4.61$ , the constant error.

The constant error for eight positions in the field of vision was thus determined for each standard from forty readings for each position making in all 320 observations for each eye, or 640 for each standard. With the three standards 40, 80 and 200 mm. 1,920 observations were made by each of the four observers, Mr. H. L. Osterud, graduate assistant in the department of zoölogy, Dr. C. E. Magnusson, professor of electrical engineering, and the two authors, C. J. D. and H. C. S.

The angles subtended by the three standards, determined at a distance of 525 mm., the distance of the fixation point from the surface of the cornea, plus 7 mm. as the distance from the surface of the cornea to the nodal point of the lens was  $4^\circ 5'$  for 40 mm.,  $8^\circ 33''$  for 80 mm. and  $20^\circ 36''$  for 200 mm.



## (d) REVIEW OF THE LITERATURE

It is not the intention of the authors to review the extensive literature of 'Augenmass.' There have been reported, however, by certain authors differences in results of the right and left lying extents which might indicate that the position in the field of vision of the standard or variable, influences the judgment of extent. It is with such authors that we are now concerned. August Kundt<sup>1</sup> in his 'Untersuchungen über Augenmass und optische Täuschungen' was the first to obtain results which point to an influence of position on the judgment of extent. His paper is divided into two parts: I. Das Schätzen der Distanzen and II. Das Schätzen der Winkel und die optischen Täuschungen. It is only with the first part that we are now concerned. The apparatus consisted of a wooden screen in which two eyeholes and a place for the nose were cut, supported upon vertical rods at the end of a table. Upon the table in such a position that the eyes of the observer looked down upon it, was a wooden block in which projecting pegs could be set and beneath which the points of a pair of compasses could be placed in such a manner that the points of the compass formed one of the limiting points of the extent and the variable middle point which was set by hand. The remaining pegs allowed comparisons to be made between filled and empty intervals. The error made in setting the movable compass point in the middle of the total extent, was determined. The eyes were used separately. The length of the total extent was 241.9 mm. The distance from the nodal point of the lens to the middle of the extent was 338 mm. The distance between the pegs was varied in different experiments. As is well known, Kundt found that the filled extent was overestimated. After establishing this result in three series of experiments, Kundt made a fourth series of observations with empty intervals. The problem was to determine the middle point of an extent 100 mm. long. The distance from the nodal point was 226 mm. and the number of observations 79. The results show a difference between the right and left eyes.

<sup>1</sup> *Annalen der Physik und Chemie*, Bd. CXX., S. 118, 1863.

	Left Eye	Right Eye
Mean.....	50.33	49.845
MF.....	0.50	0.66
Pe.....	0.05	0.07

The distances just given were measured from the right end of the extent to the apparent middle. Thus for the right eye the right lying extent was overestimated and with the left eye, the left lying extent. In both cases the extent whose retinal image fell upon the nasal retina was overestimated.

“Es bleibt nun noch der Fehler bei den Halbierungsversuchen ohne eingeschobene Spitzen zu erklären. Sind zwei einfache Distanzen gegeben, die in einer zur Sehaxe senkrechten Geraden neben einander liegen und ist der gemeinschaftliche Punkt derselben fixirt, so werden, wenn die Distanzen gleich sind, auch ihren scheinbaren Grössen genau dieselben sein, wenn wirklich das Auge eine Kugel wäre. Nun ist bekannt dass das Auge in den verschiedenen Richtungen verschiedene Krümmungen hat, es lässt sich aber auch annehmen, dass in einem einzigen, z. B., dem horizontal Schnitt, die Krümmung nicht an allen Stellen dieselbe ist. . . . Es müssen daher auch die scheinbaren Grössen der Distanzen an den verschiedenen Stellen verschieden sein. Unsere Versuche ergeben, dass die scheinbare Grösse einer Distanz auf der einen Seite eine andere ist, als auf der andern, und zwar liegen die Stellen der gleichen Schätzung in beiden Augen symmetrisch, man schätz in beiden Augen eine Distanz, die nach Aussen liegt zu klein. Die dadurch angezeigte Symmetrie in der Krümmung des horizontalen Schnittes der Netzhaut dürfte vielleicht durch den Eintritt des Sehnerven bedingt sein” (p. 138).

One wonders why a physicist should have been at pains to explain a constant error which was considerably less than the mean variation of the observations.

R. Fischer<sup>1</sup> made comparisons between straight lines situated in different parts of the field of vision. His problem was to determine the capacity of the retina to estimate the length of lines. His apparatus consisted of two flat, blackened

<sup>1</sup> ‘Grössenschätzungen im Gesichtsfeld,’ *Arch. f. Ophth.*, XXXVII., Abth. I., pp. 97-136.

strips fastened together at their middle points, so as to form a rectangular cross. The standard and variable extents were marked by iron bands. Measurements were made to 1/10 mm. The standard extent was presented in the following order: to the left, below, to the right, above and the variable to the right, above, to the left and below. Readings from greater to equal alternated with those from less to equal. The fixation point was 200 mm. from the eyes. In the results which are quoted here both eyes were used. The observations were made under two somewhat different conditions. In one case the middle point between two termini was determined; in the other case, one extent was made equal to the other. In his experiments with horizontal extents several standards were used: 11.2; 15.9; 23.8; 34.2; 48.2; 68.4; 96.0 mm. Here the middle point between two extremes was determined. A constant error of 0.79 mm. resulted as the mean of all his observations with these standards. Expressed as a proportion, the right extent is to the left extent as 100 is to 100.79. In another series of horizontal comparisons with the following standards 6.7; 11.4; 17.2; 24.8; and 42.2 mm., the result was similar to that just stated. The constant error amounted to 3.24 mm. Expressed as a proportion, the right extent is to the left extent as 100 is to 103.24. Both experiments show that the left extent was underestimated and the right extent overestimated, a result which agrees with our own observations.

Two other observers have reported differences. Highier<sup>1</sup> determined by means of the method of average error the accuracy with which a variable extent could be made equal to a standard extent. The standard extents were 10, 20, 50, 100, 150, 200 and 250 mm. in length. The right eye alone was used and the distance from the apparatus was 500 mm. The apparatus consisted of a glass plate covered with black paper in which a slit  $\frac{1}{2}$  mm. in width and 750 mm. in length was cut. The standard and variable extents were marked by vertical wires. The apparatus was so arranged that light came through the slit from behind. The dependence of the

<sup>1</sup> *Phil. Studien*, VII., 232.



constant error upon the position of the extents in the field of vision may be stated in the author's own words.<sup>1</sup>

“Was die Eigenthümlichkeiten des constanten Fehlers in Bezug auf die Raumlage betrifft (s. Tab. V.), so wiederholt sich auch hier das beim variablen Fehler Beobachtete:  $C_1$  ( $a_r' - a$ ) ist ausnahmslos bedeutend grösser als das entsprechende  $C_r$ . Ich überschätze mithin die linksliegende Normaldistanz immer mehr als die rechtsliegende—ganz abgesehen davon, ob sie verhältnissmässig gross oder klein ist. Eine analoge constante Ueberschätzung, bei monocularem Sehen, der dem benutzten Auge heteronomen Seite ist Aubert und Kundt aufgefallen, wenn auch dieselben im allgemeinen mit kleineren Distanzen als ich operirten” (p. 244).

Muensterberg<sup>2</sup> made some 20,000 observations with an apparatus the principle of which he describes as follows. “Versuchen . . . wurden an einem einfachen Apparat ausgeführt, dessen Princip folgendes war. Auf einer dunklen, zur primär gestellten Blicklinie senkrechten Fläche sollten unter jeder möglichen Variation der Bedingungen helle Punkte und Linien sich in jeder Lage einstellen und verschieben lassen und dennoch im Blickfeld völlig isoliert bleiben, so dass die bewegenden Hände und Hilfsmittel nicht gesehen würden. Unter Punkten, die ich absichtlich nicht zu klein wählte, damit sie auch bei indirektem Sehen deutlich blieben, verstehe ich im folgenden durchweg Flächen von genau 1 qmm mit einem für die Messung benutzten Nadelstichpünktchen in ihrer Mitte; unter Linien verstehe ich durchweg Flächen, deren Breite 1mm beträgt” (p. 150). Standard distances were 10, 20, 30, 40, 50 up to 200 mm. In the horizontal meridian, he found that the left extent was overestimated and the right extend underestimated. Of this difference he says:

“Am auffallendsten scheint mir für meine gesamten Resultate eine konstante Ueberschätzung der linken und Unterschätzung der rechten Grösse. Während sie bei normalen Sehen mit bewegtem Doppelauge sich als + 2.2 prozent. für die Einstellung der rechten, — 1.6 prozent. für die

<sup>1</sup> *Loc. cit.*, 224.

<sup>2</sup> *Beiträge zur Experimentellen Psychologie*, Heft 2, 125.

der linken ergibt, sehen wir unter künstlichen Bedingungen, wie der Benutzung eines fixierten Auges, den Fehler rechts bis über + 20 pro zent. steigen, und zwar ist fast immer der positive Fehler rechts grösser als der negative links, so dass zu dem Raumlagefehler, der dort positiv, hier negativ wirkt, jedenfalls noch ein zweiter Fehler kommt, der  $V$  im Verhältnis zu  $N$  stets in derselben Richtung beeinflusst. In einzelnen Fällen, wo offenbar andere Bedingungen eine Ueberschätzung der Normal distanz mit sich brachten, konnten die Fehler sowohl rechts wie links positiv werden, der Unterschied zwischen beiden blieb aber auch dann unverhältnismässig hoch und hatte dieselbe Richtung. Eine Erklärung für den einfachsten Fall, das normale Sehen, liegt nahe. Wir sind durch unsere dauernde Lese- und Schreibegewohnheit alle eingeübt, die Augen leicht von links nach rechts zu bewegen, führen sie aber nur mit einer kleinen Anstrengung in gerader Linie von rechts nach links, da wir gewohnt sind, sie beim Lesen in Bogenlinien vom Ende der einen zum Anfang der nächsten Zeile zurückschweben, Erfahrungen, die schon Purkinje und Joh. Muller erwähnen"<sup>1</sup> (p. 167).

## II. RESULTS OF OUR OWN EXPERIMENTS

The outcome of our experiments is shown in Tables I., II., III. and IV. and, graphically, in Figs. 8, 9, 10 and 11. In both tables and figures the results for each observer and for each standard are kept separate. In the tables three quantities are determined: the constant error,  $CE$ ; the mean variation  $MV$ , of a series of 40 observations; and the probable error of the series,  $PEm$ , calculated from the  $MV$  by the approximate formula already referred to. Furthermore, the results for the right and left eyes are separately stated in the tables. The positions of the standard and variable extents are described in degrees of the field of vision.

180° is the horizontal meridian with the variable on the right.

<sup>1</sup> James' comment ('Principles of Psychology,' II., 201) on this explanation is worth noting in this connection. "Now I have been a reader for more years than Herr Munsterberg; and yet with me there is a strongly pronounced error the other way. It is the rightward-lying interval which seems to me longer than it really is."



TABLE I

Position	Standard 40 mm.						Standard 80 mm.						Standard 200 mm.					
	R. Eye			L. Eye			R. Eye			L. Eye			R. Eye			L. Eye		
	CE	MV	PEm	CE	MV	PEm	CE	MV	PEm	CE	MV	PEm	CE	MV	PEm	CE	MV	PEm
180°	+1.24	1.55	.211	+1.81	1.27	.172	-3.52	3.88	.528	-3.60	1.80	.245	-10.56	3.90	.517	-5.67	4.37	.581
0°	+0.34	1.30	.177	+0.26	1.54	.210	+4.41	2.10	.286	+2.92	1.56	.212	+11.01	6.16	.823	+2.62	3.95	.524
315°	+0.19	1.17	.159	+0.21	0.91	.123	+2.13	1.86	.253	+0.46	2.08	.283	-6.94	5.60	.699	-4.01	5.73	.716
135°	+0.66	1.09	.148	+0.36	0.97	.132	+1.05	2.30	.313	+0.75	2.46	.335	-1.64	8.36	1.111	-3.38	4.27	.568
90°	+3.70	1.50	.204	+4.05	1.00	.135	+8.25	1.80	.245	+6.85	1.69	.230	-6.95	2.72	.369	-4.50	3.55	.482
270°	-3.10	0.90	.122	-2.80	0.90	.122	-5.07	1.28	.174	-5.29	1.14	.155	-30.93	2.30	.311	-31.53	2.69	.365
45°	+2.71	0.99	.134	+2.30	0.91	.123	+2.80	2.62	.357	+2.37	1.94	.264	+15.23	6.62	.887	+6.26	4.67	.572
225°	-0.30	0.92	.124	-0.78	0.72	.098	-3.38	1.19	.162	-3.53	1.29	.176	-15.00	3.97	.527	-10.57	4.21	.559

TABLE II

Position	Standard 40 mm.						Standard 80 mm.						Standard 200 mm.					
	R. Eye			L. Eye			R. Eye			L. Eye			R. Eye			L. Eye		
	CE	MV	PEm	CE	MV	PEm	CE	MV	PEm	CE	MV	PEm	CE	MV	PEm	CE	MV	PEm
180°	+1.73	1.51	.205	+1.85	1.15	.156	+6.88	3.18	.432	-0.73	2.16	.292	-11.25	1.16	.157	-11.79	7.35	.986
0°	+2.20	1.04	.141	+2.47	1.15	.156	+1.78	2.14	.289	+4.94	1.53	.268	+10.89	4.98	.614	+5.08	6.59	.883
315°	-0.93	1.43	.194	-0.15	1.18	.160	-8.21	2.59	.351	-1.96	1.94	.263	-4.18	10.24	1.317	-0.09	7.27	.975
135°	+4.96	2.15	.292	+4.74	1.41	.192	+16.18	2.50	.339	+10.32	3.20	.434	-3.43	5.79	.724	-6.39	8.80	1.121
90°	+2.75	1.38	.188	+3.09	1.24	.168	+5.31	2.22	.300	+3.10	2.42	.338	-3.21	5.91	.741	-7.30	6.45	.864
270°	-0.29	1.33	.181	-0.48	1.13	.154	+0.70	2.81	.381	+1.85	2.62	.355	+0.93	9.35	1.296	-3.91	4.15	.551
45°	+2.87	1.88	.252	+2.00	1.31	.178	+0.49	2.35	.318	+7.72	3.61	.490	+4.08	10.55	1.309	-5.33	9.88	1.268
225°	+0.92	1.74	.232	-0.26	0.99	.134	-4.66	2.65	.360	-4.55	1.95	.265	-5.77	7.91	1.050	-7.80	5.76	.720

TABLE III

Position	Standard 40 mm.						Standard 80 mm.						Standard 200 mm.					
	R. Eye			L. Eye			R. Eye			L. Eye			R. Eye			L. Eye		
	CE	MV	PEm	CE	MV	PEm	CE	MV	PEm	CE	MV	PEm	CE	MV	PEm	CE	MV	PEm
180°	+0.19	0.46	.062	-0.41	1.30	.177	-0.69	4.14	.550	+0.07	2.35	.318	-0.37	7.43	.997	-3.69	7.74	.985
0°	+0.78	1.10	.149	+1.79	1.34	.182	+2.38	2.73	.370	+1.43	1.77	.240	-4.51	8.71	1.109	-11.52	5.20	.644
315°	-0.91	1.55	.211	+0.21	1.09	.148	-1.71	2.08	.283	+3.72	2.41	.327	+11.03	8.65	1.102	+5.44	8.40	1.117
135°	+2.78	1.25	.169	+2.02	1.07	.145	-1.71	2.23	.301	-3.42	2.20	.297	-17.40	9.97	1.280	-20.89	8.53	1.194
90°	+2.48	1.40	.191	+1.68	0.87	.118	-3.82	1.73	.235	-3.08	2.93	.398	-7.92	8.23	1.094	-24.27	7.45	1.000
270°	-0.92	1.23	.167	+0.01	0.95	.129	+1.14	2.11	.285	+0.37	3.52	.478	+0.10	7.28	.977	+0.41	5.57	.694
45°	+1.06	1.10	.149	+1.82	0.81	.110	+2.87	2.76	.376	-1.47	1.74	.232	-21.27	8.84	1.127	-27.71	6.43	.861
225°	-0.42	1.12	.153	+0.11	1.13	.154	-0.03	1.87	.254	+0.56	2.55	.347	-3.56	8.45	1.124	-6.56	9.02	1.151

TABLE IV

Position	Standard 40 mm.						Standard 80 mm.						Standard 200 mm.					
	R. Eye			L. Eye			R. Eye			L. Eye			R. Eye			L. Eye		
	CE	MV	PEm	CE	MV	PEm	CE	MV	PEm	CE	MV	PEm	CE	MV	PEm	CE	MV	PEm
180°	-2.86	1.00	.135	-2.59	0.94	.128	-4.39	2.64	.358	-4.52	2.22	.300	-19.77	8.38	1.114	-26.82	7.31	1.104
0°	+3.15	1.06	.144	+3.35	0.92	.124	+4.22	2.24	.303	+5.71	3.27	.444	-24.86	6.47	.866	-23.75	5.42	.674
315°	+2.20	1.05	.142	+1.27	1.20	.163	+6.26	3.75	.509	-3.20	2.27	.306	+4.92	8.30	1.100	-0.82	9.25	1.180
135°	+0.37	1.33	.181	+0.22	1.56	.212	-2.84	4.52	.614	-2.25	2.54	.345	+2.32	7.40	.993	-0.52	5.03	.621
90°	+0.35	0.91	.123	-0.15	1.29	.176	-1.46	2.81	.381	-4.15	2.52	.342	+24.52	7.26	.974	+13.81	7.40	.993
270°	-1.02	1.33	.181	+0.62	1.39	.189	-0.33	1.79	.243	+2.40	2.20	.297	-30.49	5.46	.679	-23.55	7.20	.996
45°	+3.67	1.14	.155	+3.53	1.40	.191	+4.68	2.26	.306	+3.46	2.51	.341	-19.35	4.35	.579	-24.61	5.56	.693
225°	-1.47	1.92	.259	-1.66	1.18	1.60	-1.22	2.43	.330	-1.06	2.43	.330	+12.59	5.31	.659	+14.31	4.15	.551
													-16.49	6.10	.767	-15.47	6.59	.833
																-14.87	6.45	.864

$0^\circ$  is the horizontal meridian with the variable on the left.

$315^\circ$  is the right oblique meridian<sup>1</sup> with the variable on the right.

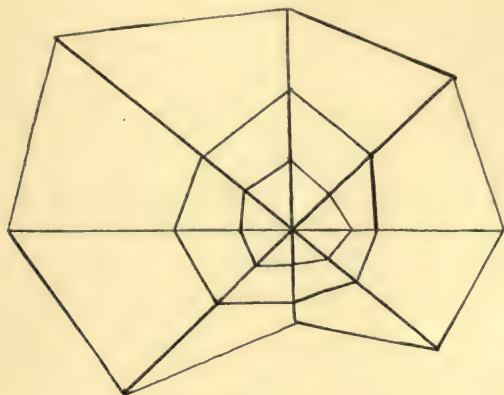


FIG. 8.

$135^\circ$  is the right oblique meridian with the variable on the left.

$90^\circ$  is the vertical meridian with the variable above the fixation point.

$270^\circ$  is the vertical meridian with the variable below the fixation point.

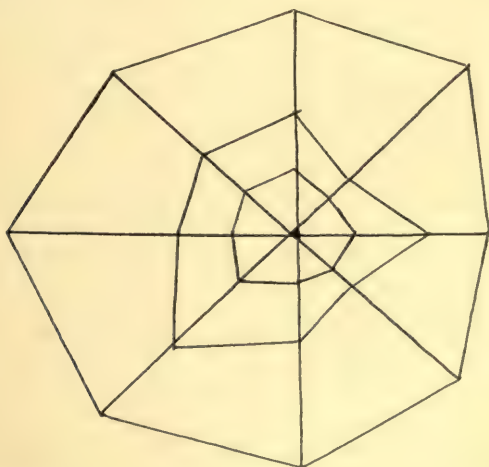


FIG. 9.

<sup>1</sup> The right oblique meridian slopes from the right, upper quadrant of the field of vision, to the left, lower quadrant, inclined  $45^\circ$  from the vertical. The left oblique meridian slopes from the left, upper quadrant of the field of vision, to the right, lower quadrant, inclined  $45^\circ$  from the vertical.

$45^\circ$  is the left oblique meridian with the variable to the left.

$225^\circ$  is the left oblique meridian with the variable to the right.

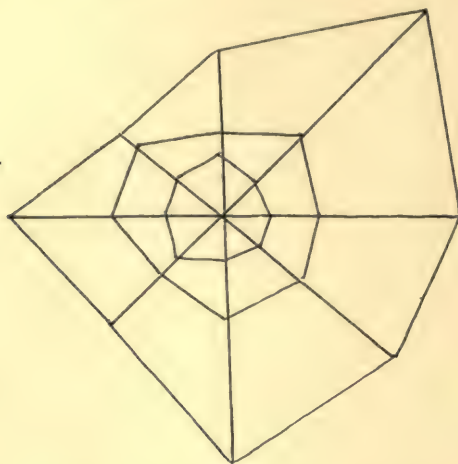


FIG. 10.

Figs. 8, 9, 10 and 11 show the positions of the meridians in the field of vision. The results of observers H. L. O.,

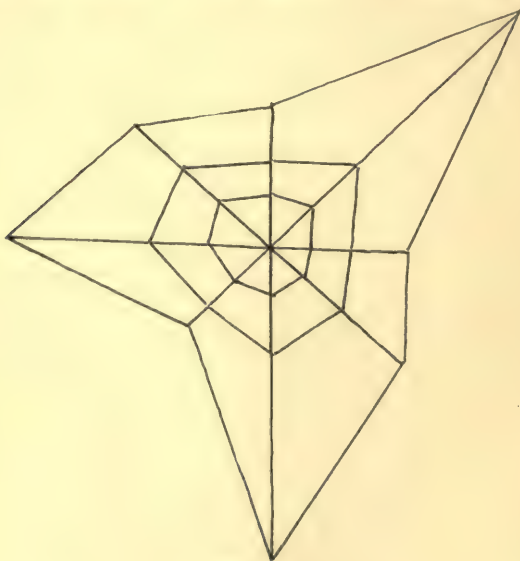


FIG. 11.

C. E. M., C. J. D. and H. C. S. are given in Tables I., II., III. and IV., respectively.



The constant error is the significant quantity in our tables. By its variation in the field of vision, the variation of the over- and underestimation of the variable becomes apparent. These variations are shown graphically in Figs. 8, 9, 10 and 11. The figures were plotted from Tables I., II., III. and IV. by reducing one fourth each of the standard distances. The 40 mm. standard thus becomes 10 mm.; the 80 mm. standard 20 mm.; and the 200 mm. standard 50 mm. To each of these reduced standards the constant error (the average of the two eyes was taken) is added or subtracted according to its sign. The ratio of the *CE* to the standard is thus magnified fourfold and the direction and amount of the variation made visibly very conspicuous.

For the purposes of analysis the results for each observer, for each standard, in each meridian, are stated in words, in terms of over- and underestimation of the variable. For the sake of brevity this statement takes the form of a catalogue.

With H. L. O., for the 40 mm. standard,

- in the horizontal meridian, the right extent is underestimated;<sup>1</sup>
- in the right oblique meridian, the right extent is slightly overestimated relative to the left extent, but is absolutely underestimated;
- in the vertical meridian, the upper extent is underestimated and the lower overestimated;
- in the left oblique meridian, the right extent is overestimated.

For the 80 mm. standard,

- in the horizontal meridian the right extent is overestimated; the left, symmetrical extent is underestimated;
- in the right oblique meridian, the right extent is underestimated; the left symmetrical extent is underestimated but less so than the right extent;
- in the vertical meridian, the upper extent is underestimated and the lower extent overestimated;
- in the left oblique meridian, the left extent is underestimated and the right extent overestimated.

For the 200 mm. standard,

- in the horizontal meridian, the right extent is overestimated; the left extent is underestimated;
- in the right oblique meridian, the right extent is overestimated; the left extent is underestimated;

<sup>1</sup> Underestimation of a variable extent means that, with respect to the standard extent, an excess of objective space had to be added to the variable in order to produce the same feeling of spatial magnitude as that produced by the standard extent. Overestimation means that a lesser spatial magnitude was able to produce the same feeling of extent as that produced by the standard spatial magnitude.

in the vertical meridian, the upper extent is overestimated; the lower extent is also overestimated and more than the upper extent;  
in the left oblique meridian, the left extent is underestimated; the right extent is overestimated.

With C. E. M., for the 40 mm. standard,

in the horizontal meridian, the right extent is underestimated; the left extent is underestimated more than the right extent;  
in the right oblique meridian, the right extent is overestimated; the left extent is underestimated.  
in the vertical meridian, the upper extent is underestimated; the lower extent is overestimated;  
in the left oblique meridian, the left extent is underestimated; the right extent is overestimated by the left eye and underestimated by the right eye.

For the 80 mm. standard,

in the horizontal meridian, the right extent is underestimated considerably by the right eye and the left extent slightly by the left eye;  
in the right oblique meridian, the right extent is overestimated; the left extent is underestimated;  
in the vertical meridian, both upper and lower extents are underestimated, the upper more than the lower;  
in the left oblique meridian, the left extent is underestimated; the right extent is overestimated.

For the 200 mm. standard,

in the horizontal meridian, the right extent is overestimated; the left extent is underestimated;  
in the right oblique meridian, the right extent is overestimated; the left extent is underestimated;  
in the vertical meridian, the upper extent is overestimated; the lower extent is slightly underestimated by the right eye and decidedly overestimated by the left eye;  
in the left oblique meridian, the left extent is underestimated by the right eye and overestimated by the left eye; the right extent is overestimated.

With C. J. D., for the 40 mm. standard,

in the horizontal meridian the right extent is slightly underestimated by the right eye and is overestimated by the left eye; the left extent is underestimated;  
in the right oblique meridian, the right extent is overestimated by the right eye and underestimated by the left eye; the left extent is underestimated;  
in the vertical meridian, the upper extent is underestimated; the lower extent is overestimated by the right eye and underestimated by the left eye;  
in the left oblique meridian, the left extent is underestimated; the right extent is overestimated by the right eye and slightly underestimated by the left eye.

For the 80 mm. standard,

in the horizontal meridian, the right extent is overestimated; the left extent is underestimated;  
in the right oblique meridian, the right extent is underestimated; the left extent is overestimated;  
in the vertical meridian, the upper extent is overestimated; the lower extent is underestimated;  
in the left oblique meridian, the left extent is underestimated by the right eye and overestimated by the left eye; the right extent is slightly underestimated by the left eye.

For the 200 mm. standard,

- in the horizontal meridian, the right extent is overestimated; the left extent is overestimated more than the right extent;
- in the right oblique meridian, the right extent is underestimated; the left extent is overestimated;
- in the vertical meridian, the upper extent is overestimated; the lower extent is underestimated;
- in the left oblique meridian, the left extent is overestimated; the right extent is also overestimated but much less than the left.

With H. C. S., for the 40 mm. standard,

- in the horizontal meridian, the right extent is overestimated; the left extent is underestimated;
- in the right oblique meridian, the right extent is underestimated; the left extent is underestimated, but less so than the right;
- in the vertical meridian, the upper extent is underestimated by the right eye; the lower extent is overestimated by the right eye and underestimated by the left eye;
- in the left oblique meridian, the left extent is underestimated; the right extent is overestimated.

For the 80 mm. standard,

- in the horizontal meridian, the right extent is overestimated; the left extent is underestimated;
- in the right oblique meridian, the right extent is underestimated; the left extent is overestimated;
- in the vertical meridian, the upper extent is overestimated; the lower extent is overestimated by the right eye and underestimated by the left eye;
- in the left oblique meridian, the left extent is underestimated; the right extent is overestimated.

For the 200 mm. standard,

- in the horizontal meridian, the right extent is overestimated; the left underestimated;
- in the right oblique meridian, the right extent is underestimated; the left extent is overestimated;
- in the vertical meridian, the upper extent is overestimated; the lower extent is underestimated;
- in the left oblique meridian, the left extent is overestimated; the right extent is overestimated, somewhat more than the left.

The significance of the results is not immediately apparent from this catalogue of over- and underestimation. If, however, the results are grouped according to the over- and underestimation of the variable extent with respect to each meridian and to each standard, the almost consistent overestimation of the right half of the *horizontal* and right *oblique* meridians, becomes apparent. The R. plus or L. plus in Table V. means that the right or left extent was overestimated in the sense explained in the footnote on page 19. Ur. plus



and Lr. plus means that the upper and lower extent was overestimated.

TABLE V

Meridian	H. L. O.		C. E. M.		C. J. D.		H. C. S.	
	R. +	Lr. +	R. +	Lr. +	R. +	Lr. +	R. +	Lr. +
Right oblique . . .	3	0	3	0	2	1	3	0
Horizontal . . . . .	3	0	2	1	2	1	3	0
Left oblique . . . .	2	1	3	0	1	2	0	3
	Ur. +	Lr. +	Ur. +	Lr. +	Ur. +	Lr. +	Ur. +	Lr. +
Vertical . . . . .	0	3	1	2	2	1	2	1

The figures in the table signify that the right or left variable was overestimated for a certain number of the standard extents. Thus H. L. O. overestimated the right extent in the horizontal meridian for each of the three standards. Again C. J. D. overestimated the right extent in the right oblique meridian for two standards and overestimated the left extent in the same meridian for one standard.

The results for the left oblique meridian show considerable divergence. Two observers, H. L. O. and C. E. M., consistently overestimate the right extent; on the other hand C. J. D. and H. C. S. as consistently overestimate the left extent. Similarly in the vertical meridian, two observers, H. L. O. and C. E. M., overestimate the lower extent while C. J. D. and H. C. S. overestimate the upper extent. Why there should be this divergence of result for the left oblique meridian, the authors are unable to explain. On the other hand, the mixed outcome of the comparisons in the vertical meridian was perhaps to have been expected, inasmuch as previous experimentation upon the same problem has not resulted in a unanimous verdict.<sup>1</sup>

### III. DISCUSSION OF THE RESULTS

It remains to consider the significance of the overestimation of the right halves of the horizontal and right oblique meridians where the results of our observations are practically unanimously in accord. It should be noticed (1) that both eyes are substantially in agreement not only with regard

<sup>1</sup> Cf. R. Fischer, *loc. cit.*

to the direction of the overestimation but also with regard to the amount. It follows from this fact that corresponding halves of the retinas have an identical space sense not only with respect to the kind of constant space error but also with respect to its magnitude. (2) Objects situated in the right half of the periphery of the field of vision will appear larger than similar objects situated in the left half of the field of vision, for the reason that the retinal images of the objects in the right half of the field of vision will be formed upon the left corresponding halves of the retina and therefore will suffer a considerable overestimation. (3) The cause of the phenomenon lies in some as yet unknown anatomical or physiological condition peculiar to the left corresponding halves of the retina in virtue of their connection with the left hemisphere of the brain. This suggestion derives weight from the mode of origin of the optic nerves. Unlike the sensory portions of the cranial and spinal nerves which arise from groups of cells which have migrated from the neural crest the so-called optic nerves arise as tubular outgrowths from the diencephalon. The distal portion of the optic vesicle by the intussusception of the bulb forms the retina; the slender elongated portion of the vesicle, the optic stalk, forms the optic nerve. There is therefore some justification for Gould's somewhat anthropomorphic notion<sup>1</sup> that 'the brain itself comes out to see.' The peculiar position of the optic nerve is clearly shown in the excerpt which follows from 'The Development of the Human Body,' J. Playfair McMurrich, p. 492. "From what has been stated above it will be seen that the sensory cells of the eye belong to a somewhat different category from those of the other sense organs. Embryologically they are a specialized portion of the mantle layer of the medullary canal. Whereas in the other organs they are peripheral structures either representing or being associated with representatives of the posterior root ganglion cells. Viewed from this standpoint, and taking into consideration the fact that the sensory portion of the retina is formed from

<sup>1</sup> 'Righthandedness and Lefthandedness,' p. 18.

the invaginated part of the optic bulb, some light is thrown upon the inverted arrangement of the retinal elements, the rods and cones being directed away from the source of light. The normal relations of the mantle layer and the marginal velum are retained in the retina and the latter serving as a conducting layer for the axis cylinders of the mantle layer (ganglion) cells, the layer of nerve fibers becomes interposed between the source of light and the sensory cells. Furthermore, it may be pointed out that if the differentiation of the retina be imagined to take place before the closure of the medullary canal—a condition which is indicated in some of the lower vertebrates—there would be then no inversion of the elements, this peculiarity being due to the conversion of the medullary plate into a tube, and more especially to the fact that the retina develops from the outer wall of the optic cup. In certain reptiles in which the eye is developed in connection with the epiphysial outgrowths of the diencephalon, the retinal portion of this pineal eye is formed from the inner layer of the bulb, and in this case there is inversion of the elements.

“A justification of the exclusion of the optic nerve from the category which includes the other cranial nerves has now been presented. For if the retina be regarded as a portion of the central nervous system, it is clear that the nerve is not a nerve at all in the strict sense of that word, but is a tract confined throughout its entire extent within the central nervous system and comparable to such a group of fibers as the direct cerebellar or fillet tracts of that system.”

Thus far we have trodden the solid ground of fact. It remains to inquire whether our result is only a curious anomaly of vision or whether so striking a circumstance as the constant overestimation of the right half of the field of vision has not resulted in some equally marked modification in behavior. Believing as we do that there can be no constant peculiarity of sensation without some equally constant form of motor response, there are many considerations which lead one to think of the predominant use of the right hand by 98 per cent. of the human species as a consequence of the differ-



ence in space sense between the right and left halves of the retinas. (1) Those highly specialized unilateral movements of the right hand and arm, which characterize righthandedness receive their innervation from the motor region of the left hemisphere of the cerebrum. In proof of this proposition one may cite (a) the heterolateral movements of the limbs which are elicited by stimulation of the motor region of the cortex cerebri of dogs and apes. (b) The occurrence of lesions in that hemisphere of the brain which is heterolateral to the side of the body upon which a hemiplegia occurs. (c) The decussation of the pyramidal tracts which can be demonstrated by gross and microscopic methods. (2) From the law of the forward direction<sup>1</sup> of the nerve impulse by virtue of which the nerve impulse flows only from afferent or associative to efferent neurones, it follows that the nerve impulse which innervates the pyramidal cells for the hand and arm must originate in some afferent system of neurones. The experimental proof of this law rests upon the fact known as the Bell-Magendie Law,<sup>2</sup> that stimulation of the central end of the cut motor root of a spinal nerve produces no motor effect. Indeed the Bell-Magendie law is only a special case of the more general law of forward direction. Psychologically the law means that there can be no motor expression without some form of sensory impression. (3) It follows from our second consideration that such a conspicuous example of motor bilateral asymmetry as righthandedness must have its cause in an equally conspicuous example of sensory bilateral asymmetry. This sensory bilateral asymmetry exists in the difference in space sense between the right and left corresponding halves of the retinas. In view of the unique mode of origin of the optic nerve and the fact that it is to be considered as a conducting tract within the central nervous system similar to the fillets, the circumstance that the left corresponding halves of the two retinas are connected with the left hemisphere directly and exclusively as is abundantly proven by hemianopsia and the histological tracing of con-

<sup>1</sup> James, 'Prin. of Psych.,' Vol. II., 581.

<sup>2</sup> Sherrington, 'The Integrative Action of the Nervous System,' 38.

duction pathways, is almost conclusive proof that the afferent system of neurones from the left corresponding halves of the retinas is the source of the current of innervation which eventually produces movements of the right hand and arm. This view is rendered the more plausible when one considers the importance of the eyes as organs for initiating manual responses and also the increasing importance of the coöperation of the eyes and the hand as the education of an infant proceeds. One would doubtless be well within safe limits in saying that in the first years of life fully 90 per cent. of all movements not instinctive are made in response to visual stimuli. Furthermore the control of all exact and refined movements, by the eyes is unremitting especially during the formative periods of such responses. All the more then would one naturally look to the eyes for the initial cause of so marked a motor phenomenon as righthandedness. (4) The manner in which such a sensory difference might operate to cause righthandedness, is easy to conceive. Let it be supposed that an infant is born with a numerical excess<sup>1</sup> of retinal elements in the left corresponding halves of the two retinas. In consequence of this numerical excess over the non-corresponding halves, those objects the retinal images of which are formed upon the left halves of the retinas appear large. It has been shown by Raehlmann<sup>2</sup> that the periphery of retina does not attain functional maturity to an extent that objects are noticed when imaged upon it until about the fifth month after birth. According to the observations of Baldwin<sup>3</sup> and those made more recently by Mrs. Woolley<sup>4</sup> a decided preference for the right hand evinced itself in the

<sup>1</sup> We suggest a numerical disparity as perhaps the most obvious cause of the difference in space sense. An assiduous search through the literature has failed to bring to light any determination bearing directly upon this point. Counts and estimates have been made, to be sure, of the retinal elements per unit area of the macular and peripheral regions. But so far as we have been able to ascertain, no direct comparison has been made of two symmetrical non-corresponding areas of equal size. Needless to say in view of the results of our experiments such a study is greatly to be desiderated.

<sup>2</sup> *Zeitschrift für Psychologie und Physiologie der Sinnesorgane*, Vol. 2, p. 53.

<sup>3</sup> 'Mental Development, Methods and Processes,' 64.

<sup>4</sup> 'The Development of Righthandedness in a Normal Infant,' Helen Thompson Woolley, *PSYCHOLOGICAL REVIEW*, XVII., 37.

seventh month. We believe that the latter phenomenon depends upon the former. If an infant at the time that the periphery of its retina were maturing, were confronted by two objects of approximately equal size, both lying in the horizontal plane at the same distance from the eyes, the right object would form its retinal images upon the left corresponding halves of the retinas, while the left object would form its retinal images upon the right corresponding halves of the retinas. The right object by appearing larger would make a stronger claim upon attention. Once attended to, that reflex connection between the periphery of the retina and the rectus internus of the right, and rectus externus of the left eye would move the eyes until the object lay in the direct regard of the foveas. Following the eye movements, the reaching movements of the arm and hand of the right side naturally ensue. The hand thus favored gains in skill and comes naturally to be used in all movements requiring superior precision and control. Finally, practice and habit make the reaction inveterate.

The theory here outlined was first published three years ago. Since that time, two experimental studies have appeared which corroborate strongly the conclusions which have been arrived at in this paper. Woolley<sup>1</sup> made a series of observations upon an infant beginning in the middle of the seventh month of its life. Colored discs and a wooden square and circle were placed before the infant at distances within easy reach. The number of times the right or left object was reached for and the hand by which the movement was made, were recorded. The results may be stated in the words of the author. "In recording which of the discs or forms was taken, it soon became apparent that regardless of the hand used, the right hand position possessed an independent attraction for the child. In the selection of the colors, where the right hand was used 206 times and the left 194, and both 68, the right hand *position* (italics in the original) was selected 285 times, and the left 183. The difference is even more marked in the case of the square and the circle, where the material itself was indifferent. Out of

<sup>1</sup> *Loc. cit.*



70 choices, 56 were for the right hand position and only 14 for the left. . . . I must confess myself at loss for an explanation for this preference for the right hand position. It could not have had to do with the child's posture, or her relation to the light, for those factors were varied from day to day. The tests were not all made in the same room, and the child sat sometimes in a high chair, sometimes on a couch, and sometimes on the floor. The only requirement was that she should be in an easy position, with both arms free to move. The only suggestion which seemed at all probable was that it might be conditioned by her eyes, though as far as I could judge, or could test them her eyes seemed not only normal, but unusually good. They were very well coördinated at an early age, and gave more evidence of distant seeing than is usual at so young an age. So far as their application to theory is concerned, the tests are only one more proof of the already accepted view that righthandedness must be a normal part of physiological development, not a phenomenon explicable by training. The preference for the right-hand position, in excess of that for the right hand (the author means the right object was sometimes reached for and grasped by the left hand) suggests a query as to whether the eyes could play a rôle in the development of right handedness, but the query is scarcely worth making so long as the observation is an isolated one."<sup>1</sup> It would be difficult to imagine stronger corroborative evidence that the development of righthandedness depends upon the eyes. It is not shown, however, that the reason for the preference of the right *position* apart from the nature of the object, was due to a difference of the space sense of the peripheries of the retinas. The confirmation of the requirements of the theory is the more striking in view of the fact that the authoress was unaware that such a theory had been suggested.

Poschoga<sup>2</sup> determined the absolute space limen for eight radii of the field of vision and for two parallels of latitude

<sup>1</sup> *Loc. cit.*, p. 40.

<sup>2</sup> 'Die sukzessive und simultane Raumschwelle im indirekten Sehen,' Nicolai Poschoga, *Wundt's Psych. Studien*, VI., 384.

( $22.5^\circ$ ) and ( $45^\circ$ ). The radii were  $45^\circ$  apart. Those points in the field of vision were investigated which lay at the intersections of the radii and the parallels of latitude. Using these points as centers, the limen was determined both by the simultaneous and successive presentation of stimuli for vertical, horizontal and two oblique dimensions. The limina are somewhat different in the two cases. In both however marked differences exist between the threshold in the right and left halves

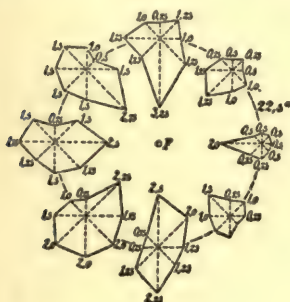


FIG. 12.

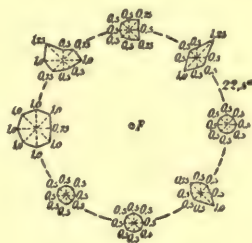


FIG. 13.

of the field of vision. Figs. 12 and 13 show Poschoga's limina for the  $22.5$  degree parallel.

Only the results obtained by Poschoga at  $22.5$  degrees are taken into consideration by the authors. They feel from their own observations that reliable readings can not be made at forty-five degrees from the fixation point. Poschoga indeed admits as much himself. He says:<sup>1</sup> "Auf der nasalen Seite ist nämlich die Deutlichkeit des Sehens in diesen äusseren Regionen so gering, dass man der Lichtreize nicht direkt von der diffusen Reflexion des Lichtes an dem Nasenrücken zu unterscheiden vermag. Ich habe erst später gewissermassen zufällig festgestellt, dass ich die Lichtquelle selbst bei jenem Punkt nicht mehr sehen konnte. Insofern scheinen die Werte für die Schwellen in diesen äusseren Region der nasalen Seite mehrdeutig zu sein." In the absence of any numerical criterion of the accuracy of his results and with the knowledge of the difficulty of exact observation in the periphery of the field of vision, one feels justified in saying that doubtless

<sup>1</sup> *Loc. cit.*, 409.

Poschoga's comment on his own results at forty-five degrees on the nasal side should be extended to those of the temporal side as well. Although there is considerable discrepancy between the two results in many concrete instances, there is, notwithstanding differences, a very general agreement, in the fact that the limen in the left positions of the field of vision is very much greater than the limen of symmetrical positions in the right half of the field of vision. The authors consider this circumstance but another demonstration of the fact already shown in this paper and elsewhere<sup>1</sup> that objects in the right half of the field of vision are overestimated. This overestimation of the size of an object is due to some anatomical peculiarity of the left corresponding halves of the retina which they acquire by virtue of their connection with the left hemisphere of the brain. A fact analogous to this relationship between a small spatial limen and an increased perception of size is the well-known tactual space illusion which occurs when a pair of compass points separated by 1 cm. are drawn from the ear on one side of the face across the lips to the opposite side of the face. Although the points of the compass trace a pair of parallel lines over the skin, the apparent course of the points is two lines concave above and below the mouth. Similarly, two compass points set at a fixed distance apart and drawn down the volar surface of the forearm upon the palm, seem at first to trace a single line but appear to separate when the palm is reached.<sup>2</sup> There seems therefore to be good grounds for the generalization that in space perception those parts with the finest discrimination of two points are also the parts that give the greatest perception of size. If one compares Poschoga's figures<sup>3</sup> of the limina which are reproduced in Figs. 12 and 13 with Figs. 1 and 2 of Stevens's article<sup>4</sup> on the perception of size by peripheral retina, the extent to which one figure supplements the other becomes very striking. Those positions in the field of vision which show the smallest limen in Poschoga's figures show the greatest overestimation in those of Stevens.

<sup>1</sup> Stevens, 'Peculiarities of Peripheral Vision,' *PSYCH. REV.*, XV., 69.

<sup>2</sup> See James, 'Prin.,' II., 141 and 142, for figures showing these illusions.

<sup>3</sup> *Loc. cit.*, pp. 74 and 75.

<sup>4</sup> Fig. 12 is the *sukzessivschwelen*. Fig. 13 is the *simultanschwellen*.



SUMMARY OF RESULTS

1. With the few exceptions which have been already stated, it may be said that the right half of an extent in the field of vision is overestimated.

2. This overestimation holds true for both right and left eyes.

3. The extent which is overestimated forms its retinal image upon the left corresponding halves of the two retinas.

4. The left corresponding halves of the two retinas are connected exclusively with the left hemisphere of the cerebrum.

5. By reason of the fact of a marked difference in the space sense of the two halves of the retina, those objects in the right half of the field of vision, by appearing larger, attract the visual attention which in turn leads to grasping movements of the right hand. The hand thus favored by earliest experience acquires a special skill which causes it to be used in all manual acts requiring the greatest precision.

# DIFFERENCE-SENSIBILITY FOR RATE OF DISCRETE IMPRESSIONS<sup>1</sup>

BY KNIGHT DUNLAP

## I. THE PROBLEM AND THE APPARATUS

When two regular series of stimuli (strokes of a hammer, or flashes of light, for example), are addressed in succession to a subject, how great must be the difference in the rate of succession of the stimuli in the two series, in order that the subject may perceive that one series is faster than the other?

This question is important *per se*, but it is perhaps of greater importance in its bearings on the acceptance or rejection of certain types of apparatus which may be or may have been provisionally assumed to be suitable for experimental work on rhythm or on some other topic which requires a certain temporal uniformity of stimulation. We can not say how far below the threshold of difference-perception the variations in a given series of stimulations must be in order that the series may be said to be practically uniform, nor can we assert that variations which are above the threshold do or do not render a given series unsuitable for a certain purpose, until we have made further investigations into the effects of these irregularities upon the processes which it is proposed to attack experimentally with the aid of such series. Before, however, any conclusions in regard to points of this kind are possible, we must know approximately the ranges in which the rate-thresholds lie.

In the experiments here reported, I have attempted to determine the rate difference threshold for visual and auditory stimuli, for two standard rates, one a trifle faster than four stimulations per second, and the other a trifle faster than two per second, both when the impressions were rhythmically grouped and when they were not. I have also attempted

<sup>1</sup> From the Psychological Laboratory of the Johns Hopkins University.

in a few cases to compare the rate-threshold with the time-threshold for standard intervals corresponding to the standard rates, and have investigated superficially the effect of intensity of stimulus on the rate-judgment with auditory stimulus. I have in addition carried out one set of experiments with electrical stimulus.

The exact details of the experiments were to a considerable extent determined by instrumental limitations. The principal desiderata in the line of apparatus were: a control-instrument which should be extremely accurate in its period, and at the same time permit rapid and accurate change of its period; and stimulus instruments which should admit of perfect temporal control by the control-instrument, and which should emit stimuli of constant intensity. I found none of these conditions easy of fulfillment.

The most satisfactory control-instrument which I could secure was a weighted-spring vibrator which was constructed for these experiments. Figure 13 gives a side-view of this instrument, omitting some of the details of the switch. Figure 14 gives a view of the switch on a larger scale, looking down from above it. The letters *U*, *C*, *N*, *R*, *F* and *T* on the one figure indicate the same details as the same letters on the other figure. The spring (*V*) is a tempered steel rule, 30 cm. long, 5 mm. wide and 1 mm. thick. This spring is held in a vertical position between the plate (*Q*) and the base (*B*). The weight (*W*) is slotted to slide on the rule, and is held in any desired position by the set-screw (*P*). Two weights were used, giving two standards with the variables corresponding. The lever (*L*) of the electric switch projects at right angles to the rule and is so adjusted that it just touches the rule when the latter is at rest, being held in position by the light spring (*S*). The slightest movement of the rule from the position of rest towards (*U*) therefore breaks the contact between the lever and the screw (*J*), and it remains broken until the rule passes the point of rest in the opposite direction. The whole switch may be moved towards or away from the rule (*V*) by loosening the clamp-screw (*H*), allowing the rod (*N*) to slide through the split support (*R*).



The position of the lever (*L*) alone may be changed by adjusting the contact-screw (*J*). The whole key may be moved vertically by loosening the clamp-screw (*C*) which passes through a slot extending almost from top to bottom of the plate (*U*). The tension of the coil spring (*S*) is adjustable by turning the windlass-peg (*T*). The pieces (*E, E*) carrying the contact screws (*J, K*) are insulated from the frame (*F*), and electric connections were made at (*E*) and (*F*) by binding posts (not shown). The tips of the screws (*J, K*) and the portion of the lever (*L*) touching each are platinized. Contact (*K*) was not used in the experiments herein described. The inner ends of the thumb-screw (*D*) and its mate are cupped to receive the coned ends of the pivot to which the lever (*L*) is fastened. The lever and pivot shaft together weigh 1.7 grams, of which about two thirds is the weight of the shaft. *Z, Z, Z* indicate lock-nuts. No electric current passes through the rule (*V*); this is an important point.

The rule is set in vibration by pressing it lightly against the stop (*X*), and then releasing it. The pressure is applied to the rule by the finger, opposite (*X*), so that the rule is always flexed in the same way as well as to the same degree. When handled in this way the rule vibrates with a period which is practically constant, and which shows no progressive change during the first fifteen seconds of vibration. But it is essential that the initial amplitude shall be constant and small, or serious variations arise. The initial deflection in the experiments described below was approximately one centimeter.

The vibrations were not absolutely regular, even when all possible precautions were taken. Although there was no progressive variation in the period of the rule during the time for which the vibrations were employed in the experiments (never so long as ten seconds), slight irregular variations occurred occasionally, amounting, with the heavier weight, to from two to three sigma as a maximum (about two thirds of one per cent.). With the lighter weight the variations were so far below one sigma that they were not measurable. The vibrator, in short, seemed sufficiently accurate for the

experiments planned, and is undoubtedly much more accurate than a magnetically driven reed. A weighted reed is as treacherous as the traditional broken one, and will not vibrate uniformly except under uniform conditions.

The settings of the weights for the standard rates were at 25 on the scale of the rule. The settings for the variable were at intervals of two millimeters above and below this mark. The marks on the scale are about 0.15 mm. wide, and the settings could be made with an error of less than the width of a mark, if five seconds were allowed for the setting. It was not found practicable to allow less than five seconds, although the setting was often done in three seconds. The possibilities were thus limited to a standard rate (or time-interval) followed by a variable after a pause of five seconds. Two reeds might be used, one for the standard and one for the variable, with a considerable lessening of the experimenter's labor, but this would introduce the possibility of greater inaccuracies, and hence was not attempted.

The vibrator was standardized, with the switch adjusted exactly as used in the experiments, by means of the Pfeil marker and 250 d.v. fork writing on a drum. The marker was connected in series with the switch, and the timing was from break to break of the circuit, under which condition the Pfeil marker gives almost absolutely accurate records. Some records were also taken by the spark method (using only break-sparks) and these agreed perfectly with the Pfeil records. No account was taken of temperature changes during the course of the experiment, and these changes, although slight, undoubtedly caused some variation in the vibration period of the rule. Since these changes affected standard and variable alike (or nearly so), and were practically equivalent to a shifting of the whole scale up or down, they were of no great consequence.

The sound-source finally selected was made from a Western Union pattern telegraph sounder. It is represented in Figs. 15 and 16. The armature was removed from the sounder, and replaced by a shorter one (*A*). Short pieces of soft iron (*Y*, *Y*) were screwed on to the poles of the magnet

( $M$ ,  $M$ ); they are in such positions that the armature just moves between them without touching. The ends of the armature and the inner ends of the pole pieces were beveled, to a width of about a millimeter.

When current is allowed to flow through the sounder magnets, the armature ( $A$ ) is drawn down into line with the two pole pieces ( $Y$ ,  $Y$ ), thus depressing the long end of the lever ( $G$ ). This takes place noiselessly, as the armature is checked by the magnet. It is true that the snap of the magnet cores as they are magnetized can be heard if the ears close to the sounder (even when the armature and lever are entirely removed this is audible), but at the distance at which the subject was placed in the experiments it was not perceptible.

When the current through the magnet is interrupted, the armature and long end of the lever ( $G$ ) are allowed to rise, the short end of the lever being pulled down by the spring ( $O$ ), until the long end strikes against the screw ( $I$ ). This screw is platinum tipped and bears against a platinum plate on the lever. The use of this contact is described below.

From the description, it is perhaps clear that the sounds were produced by the armature-lever striking the screw on the up-stroke, the intensity of the stroke being regulated by the tension of the spring ( $O$ ), and by the adjustment of the screw ( $I$ ). The temporal regularity therefore depends on the break of the current through the magnets, and this is controlled almost absolutely by the vibrator. The make of a current through a magnet coil can not be depended on, if the contacts of the control instrument are of the usual type, since the current does not always rise to its maximum at the same rate.

Variations in the intensity of the current are practically without influence on this sounder, provided the weakest current is sufficient to pull the armature completely down. Theoretically, a sudden change in the current strength should modify the length of the interval in which it occurs, but practically such a change must be relatively large to produce a measurable effect, and moreover, is not apt to occur. A change in the



current from series to series (due for instance to the running down or recharging of the batteries) produces absolutely no effect. In the experiments in which the sounder was used, the current (supplied by chloride accumulators) was kept constant in amperage.

If the sounder is started and stopped by means of a simple switch in the circuit, it is apt to 'limp' on the final stroke and sometimes on the initial stroke. If the circuit is broken while the vibrator switch is closed (while therefore the sounder-lever is down), the lever will be released immediately, giving a stimulus after an interval shorter than the preceding intervals. If the circuit is completed (by the hand switch) a few sigma before the vibrator switch is opened, the sounder lever is not pulled completely down before being released, and the first stroke is weaker than the succeeding strokes, while the first interval is unduly long. The initial 'limp' occurs very seldom, as might be expected from the conditions which produce it. The terminal 'limp,' however, is a serious matter, occurring frequently if a hand switch is employed, and disturbing the subject's judgment when the limp occurs at the end of the standard series.

In order to eliminate the terminal 'limp,' the sounder-circuit was made and broken by a Western Union relay. The magnet-windings of the relay were connected in series with the contact between the lever (*G*) and the screw (*I*) of the sounder, and with a hand-switch. The contacts of the relay were so connected in the circuit through sounder-magnet and vibrator switch that when the armature lever of the relay was released by the relay magnet, it completed the sounder circuit. The sounder circuit therefore was broken by completing the relay circuit, and this could be completed only when the sounder lever was at the top of its stroke. If the hand switch in the relay circuit was closed while the sounder lever was being held down, the sounder circuit was broken immediately after the next up stroke.

As a visual stimulus the flash of a helium tube was used. The vibrator switch was connected in series with the primary of a small induction coil and with the magnet of an automatic

cut-out, which shunted the make spark around the helium tube and allowed the break spark to pass through it.

The stimulators (sounder and helium tube) were set up in one room, and the operating mechanism was installed in another room separated from the first by a hallway. In the auditory experiments the subject sat about four feet from the sounder, back to it. The helium tube was suspended, in a box open at the bottom, over a table in front of the subject. A black cloth was placed on the table, and upon this a piece of white paper three by five inches, with its long axis horizontal and its short axis inclined at an angle of about thirty degrees from the line of regard. The paper was approximately three feet from the subject's eyes. In the visual experiments the room was partially darkened, and the periodic illumination of the piece of paper came out vividly.

In order that the experimenter might hear the auditory stimulus, a microphone was fastened to a board upon which the sounder was set. The microphone was a rude affair, constructed of two electric light carbons, but worked very well. It connected with a telephone receiver suspended near the ear of the experimenter. It was necessary that the experimenter should hear the auditory stimulations in the 'time' series, in which but two stimulations were given in succession. The microphone also served a useful purpose in the experiments on intensity-changes; in these experiments the subject sat in the experimenter's room and listened to the telephone, instead of to the sounder direct, as in the other auditory experiments.

A telegraph key was placed on a small table at the right hand of the subject. This key was connected with a sounder in the experimenter's room, and by means thereof the subject communicated his judgments to the experimenter in a conventional code.

The current which operated the relay circuit, that which operated the microphone, and that which operated the telegraph system was drawn from the lighting circuit, through appropriate resistances and reduced to proper voltages by shunt resistances.

## 2. THE METHODS OF THE EXPERIMENTS

There were two main types of experiments. In experiments of the first type a series of stimulations was given at one of the standard rates during a period of approximately five seconds. Then followed a five seconds pause, during which the weight of the vibrator was set for one of the variable rates, and stimulations at this rate were given for five seconds. The subject was required to determine whether the variable series was faster than, slower than, or equal to the rate of the standard. (No distinction was made between judgments of 'equal' and judgments of 'not different.')

In some of the experiments both standard and variable series were auditory. In other cases both were visual. In experiments of a third sort one series (standard or variable) was visual and the other was auditory. In one set of experiments electric stimuli were used.

In most cases the intensity of the stimuli in the standard and variable series was the same. In certain sets, however, the intensity of the variable was greater or less than that of the standard. These were the sets in which the subject listened to the telephone (microphone) instead of to the sounder. By means of a Morse key closing a shunt circuit with appropriate resistance a part of the current flowing through the microphone could be diverted from the telephone receiver, thus decreasing the loudness of the telephone noise. The subject, during these intensity-experiments, sat at a distance of about four feet from the table on which were the vibrator and accessories, with his back towards it, and the telephone receiver was supported at a distance of about six inches from his left ear. The only feature of the operation of the apparatus (except the microphone) which was audible to the subject was the faint click of the relay as it made and broke the sounder circuit. The sounder of course was not directly audible.

There was a marked difference between the two intensities of sound which were used. The three subjects who served in this part of the experiment each estimated the louder sound to be about twice as loud as the weaker ones, on the average.



No attempt was made to measure the intensities physically (this measurement is impossible for such sounds<sup>1</sup>), but the currents which flowed through the telephone receiver when the microphone was at rest were kept constant, the amperage for the weaker series being exactly half that used for the stronger.

The series of stimulations in the intensity experiments departed from uniformity in several ways. In the first place the sounds were prolonged, each one lasting nearly to the beginning of the next one; but the duration perceptibly varied. In the second place, the sounds, as wholes, varied perceptibly in intensity, although the variations within any series were small as compared with the differences between the loud and weak series. In spite of these irregularities there was a definite regularity in the series, due, no doubt, to the equality of the intervals from the beginning of one sound to the beginning of the next.

In some of the experiments on rate-comparison, the rates were compared without grouping of the impressions. In other experiments the subjects grouped the impressions in twos, threes, fours, or sixes.

In experiments of the second type, a standard time-interval marked off by two clicks of the sounder was compared with a variable interval marked off in the same way. In some cases the standard interval was given but once, and the variable also was given but once. In other cases each was given three times, the time from the beginning of the first presentation to the end of the third being approximately

<sup>1</sup> The amplitudes of vibrations of the telephone diaphragm might be measured, as recommended by Pillsbury, *PSYCHOL. REV. MONOG. SUPPL.*, 13: 1 (53), 5-20, but this procedure would be of little assistance. Even if the diaphragm were mounted without enclosed air-space on either side of it, and were vibrating in a strictly sinusoidal way, the relative amplitudes of excursion of any single point on the diaphragm would not represent the relative amplitudes of the air waves propagated from the diaphragm. As the telephone diaphragms are actually mounted, the amplitude of vibration of the center of a given diaphragm does not even determine the amplitude of the wave it is propagating; the amplitude of the wave may be increased or decreased enormously without altering materially the limits of the diaphragm vibration, or the form of the air-wave. Finally, and not least in importance, the wave-form changes materially when heavier current is used, and the relative amplitudes of two air waves of different forms do not indicate the relative total energy of the waves.

five seconds. In either case, five seconds pause occurred between the standard and the variable, as in the rate experiments. A warning signal was given two seconds before the beginning of the standard, the signal being the extinguishing of an electric light which illuminated the small table at the subject's right hand.

In experiments of both types, the subjects were given the information that the interval between the standard and the variable was always approximately the same, but that the duration of the standard and the variable series in the rate experiments were only uniform in the average, being sometimes shorter, and sometimes longer than the nominal five seconds duration. (Except in the case of the slower standard with six-grouping. In this case the standard always consisted of twelve stimulations, and the variable of twelve or more.) The subject therefore understood that counting the stimulations would not help in the comparison of the series, and the testimony of all of the subjects was that they were entirely undisturbed by and inattentive to the numerical factor.

The experimenter controlled the durations of the rate series and of the intervening pauses while watching the second-hand of a watch placed conveniently close to the vibrator. The finger movements for discontinuing the standard and for commencing the variable were made each as the second hand crossed a five-second mark; the extreme limits of variation of the pause were therefore one stimulus-period longer and shorter than the nominal five seconds. The finger-movements for beginning the standard and for discontinuing the variable were made within limits of one second before and after the five-second marks.

The time-series were given by closing the switch and then opening it as soon as the first stimulus was heard in the telephone. In this way single pairs of stimuli were given. The pause between standard and variable was perhaps a little more variable than in the rate series; but it was not noticeably so in terms of the movement of the second-hand.

The two standard rates used were given by setting the

weight (the light one in one case and the heavy one in the other) at the 25 mark on the rule (vibrator). The actual interval between successive breaks of the switch contact was 232 sigma with the one weight, and 435.5 sigma with the other. The variables were determined by setting the weight at even-millimeter marks above and below the standard (*i. e.*, 24.8, 24.6, 24.4, etc., descending; and 25.2, 25.4, 25.6, etc., ascending). The difference in period between the rates at successive points in this scale was 2.75 sigma for the 232 standard and 7.4 for the 435.5 standard. To be more precise: the differences lay between 2.5 and 3 sigma for the one standard, and 7.25 and 7.75 for the other standard, averaging 2.75 and 7.4 respectively. I did not attempt to measure closer than 0.5 sigma in any case. Throughout the range of the scale which was used the 2 mm. step gave the same period-difference, within the limit of the measurements; that is, there were no detectable variations, although at a greater distance from the 25 mark the same step (2 mm.) gave a different period-difference.

The order in which the variables were given in any series was determined by chance, and the method<sup>1</sup> of determining the order was fully explained to each subject. The number corresponding to each rate was written on each of eight playing cards of the same denomination. The standard, for example, was represented by the queen. Playing cards were used because of their easy manipulation, and the use of one denomination for one rate made easy the sorting of the cards when less than the full number were used.

For the series in which there were but five rates all the cards for each rate were ordinarily used. In the longer series, only four of each denomination were used. Occasionally, some other number of cards was used, to make up a series to

<sup>1</sup> I describe this method in detail because it is the method of right and wrong cases, with the essential features of the classic method left out — Hamlet with the mad prince omitted. I do not know of any name by which to designate it at present. 'Method of constant stimuli' applies as well to 'method of minimal change' as to this method, and 'method of right and wrong cases' implies the mathematical treatment which was included in this method by the psychophysicists, and which I believe is entirely out of place in the treatment of psychological data of any intrinsic value.



bring a set up to the number of judgments desired, or for cases in which only a part of an experiment period was devoted to a set, as in sets O-IV. to O-VII. (see below). The normal series with eight variable rates therefore consisted of 32 variables, and the series with five rates consisted of 40 variables.

The cards were thoroughly shuffled, and the order of the variables was then drawn off in a typewritten column on a sheet of paper which was used in the experiment. The judgments were recorded on this sheet in a second column, parallel to the column of variables. On another part of the sheet the variable rates employed appeared in a short column in order of their position in the scale. After the experiment was finished the judgments were posted to this column, and posted from this to the sheet including the records of the whole set. By observing this routine all possibilities of errors in the conduct or the record of the experiment series were avoided.

The subject, being conversant with the method of making out the form-sheet for the experiments, understood that there were a number of appearances of each variable rate, and that they might appear in any order. He was not told the exact number of variable rates employed in any set. He was told that some of the variables were exactly the same in rate as the standard, and that the number of variables slower than the standard might be greater or less than the number faster than the standard. Under these conditions the subjects were able to give judgments which seemed absolutely unbiased.

In most cases, the stimuli series were given with fifteen seconds interval between the variable of one pair and the standard of the next. In some cases the interval was twenty seconds or longer, and in a few cases it was only ten seconds. To run through an experiment series of 32 pairs required usually about twenty or twenty-five minutes. In cases in which two experiment series were taken in one day, from five to fifteen minutes rest was allowed the subject between the two series. Two experiment series could be taken in this way without appreciable jading of the subject.

When a new set was begun on a subject, no records were made on the first day; the work on this day was simply devoted to familiarizing the subject with the conditions of the experiment, and to finding out the range of variables which it seemed advisable to use under those conditions. In such work the formal plan of experimentation was not adhered to, but the variables were selected currently. The one exception to the above statement was in the case of set I-III. (Subj. *MW*), which were under exactly the same conditions as the preceding series and hence called for no practice.

### 3. THE RESULTS OF THE EXPERIMENTS

The results of forty-one sets are included in the table, and the data from 35 sets are schematically presented in the graphs of Figs. 1 to 12. We must consider now the method of elaborating the data.

Each set of experiments brought out a number of judgments of the variable rates employed in the set, these judgments being distributed in the categories of 'longer,' 'equal' and 'shorter' for the time experiments, and 'slower,' 'equal' and 'faster' for the rate experiments. We may designate the judgments of 'longer' and 'slower' as *minus*, and the judgments of 'shorter' and 'faster' as *plus*. The problem is to present these results in such shape that ready comparison of the sets will be possible.

We might plot the error-curve (or a graph corresponding to it) by the orthodox method of dividing the judgments of 'equal' equally between the categories of 'plus' and 'minus,' and designating the one of these categories which contains the fewer number of judgments for a given variable the category of 'error' for that variable. The error method has absolutely no advantages over a simpler method in the way of clearness or significance in the presentation of the data we are considering, and is in the opinion of the writer decidedly objectionable in psychological work. We shall therefore turn to the simpler and more significant method.

That which interests us in the total group of judgments on any of the variables is *the percentage of cases in which it*

is discriminated from the standard. This percentage may be computed by finding the difference between the number of judgments of 'plus,' and the number of judgments of 'minus,' and reducing the difference to per cent. of the total number of judgments. The judgments of 'equal' are precisely judgments of 'no discrimination,' and an equal number of 'plus' and 'minus' judgments is the virtual equivalent of twice the number of 'equal' judgments. The percentages of discriminations are given in the table, with the signs (*plus* and *minus*), to indicate the direction of discrimination. The graphs of Figs. I to II are drawn by laying off distances on the *X*-axis which correspond to the variables, and erecting at the point corresponding to a given variable the ordinate of length proportionate to the percentage of discrimination for that variable. The variable which is equal to the standard is at the intersection of the *X*-axis with the prolongation of the two short vertical lines (these lines therefore mark the *Y*-axis), and the abscissas of the shorter (faster) variables are indicated on the minus side thereof, the longer (slower) variables being laid off on the plus side of the origin. The horizontal lines mark the loci of the points whose ordinates are 50 and 100 per cent. of discriminations, those above the *X*-axis being discriminations as 'shorter' or 'faster' (*plus* in the table), and those below the axis being discriminations as 'longer' or 'slower' (*minus* in the table).<sup>1</sup>

Fifty per cent. of discrimination is the conventional 'threshold.' This value corresponds to seventy-five per cent. of 'right' judgments (twenty-five per cent. 'error') in the error method: for when  $p - m = (p + e + m)/2$ ,  $p + e/2 = 3(m + e/2)$  or  $4(m + e/2) = p + e + m$ .

Figure 12 reproduces a graph plotted by the method above described, and the graph plotted by the error method from the same data (the data of set 7-III.).

<sup>1</sup> There is obviously an inconsistency in the choice of the plus and minus directions in the figures. Since the variables slower than the standard are plotted on the plus of the *Y*-axis, the discriminations of the variables as slower should have been plotted on the plus side of the *X*-axis: the graphs then would run upward from left to right, instead of downward. Inasmuch as the figures were wrongly drawn before this was written, and seem as intelligible in one position as in the other, the text was simply changed to agree with the figures.



TABLE OF PERCENTAGES OF DISCRIMINATIONS

Subj.	MW	MW	MW	CW	CW	CW	CW	CW	CW
Set.	1-I.	1-II.	1-III.	3-III.	3-II.	3-I.	4-II.	4-I.	4-III.
Mode.	Aud.	Aud.	Aud.	Aud.	Aud.	Aud.	Aud.	Aud.	Aud.
Type.	Rate	T-s	T-s	Rate	T-t	T-s	Rate	T-s	3's
Std.	232	232	232	232	232	232	435.5	435.5	435.5
No.	25	25	25	44	40	45	40	50	40
-4	100	80	84	97.7	85	95.5	100		
-3	100	92	96	97.7	77.5	82.2	97.5	84	
-2	96	68	40	88.6	57.5	64.4	85	82	97.5
-1	52	48	56	77.2	37.5	22.2	45	58	82.5
0	4	8	-12	11.3	-5	2.2	17.5	36	40
1	36	28	44	13.6	17.5	13.3	-25	24	-40
2	-72	-72	-28	-45.4	-25	-17.7	-65	2	-92.5
3	-100	-56	-56	-81.8	-25	-57.7	-95	-34	
4	-96	-76	-80	-95.4	-27.5	-64.4	-92.2	-46	
5					-45			-56	
6					-57.5			-78	

Subj.	CW	CW	CW	CW	CW	CW	CW	CW	CW
Set.	0-I.	0-II.	0-III.	0-IV.	0-V.	0-VI.	0-VII.	3-IV.	2-I.
Mode.	Aud.	Aud.	Aud.	Aud.	Aud.	Aud.	Aud.	Aud.	Aud.
Type.	4's	6's	2's	2's	2's	4's	6's	<sup>1</sup>	S-W, 3's
Std.	435.5	435.5	435.5	232	232	232	232	232	232
No.	40	30	40	20	20	20	20	60	25
-2	92.5	100	100	100	100	100	95	100	96
-1	80	93.3	97.5	90	70	80	90	80	60
0	35	23.3	35	5	40	45	50	30	20
1	27.5	23.3	27.5	30	30	40	25	33.3	32
2	80	73.3	90	85	95	100	85	93.3	72

Subj.	CW	GW	GW	GW	GW	GW	GW	GW	SW
Set.	2-II.	5-I.	5-II.	5-III.	5-IV.	5-V.	6-I.	6-II.	9-III.
Mode.	Aud.	Aud.	Aud.	Vis.	A.-V.	V.-A.	Aud.	Aud.	Aud.
Type.	W-s, 3's	T	Rate	Rate	Rate	Rate	S-W	W-S.	Rate
Std.	232	435.5	435.5	435.5	435.5	435.5	435.5	435.5	232
No.	25	40	40	30	40	40	25	25	40
-6					82.5				
-4					42.5	100			100
-3		75		40					92.5
-2	92	65	100	45	-15	92.2	96	100	77.5
-1	76	50	65	12.5			44	72	35
0	24	42.5	20	-27.5	-70	-12.5	-12	32	20
1	-32	2.5	-42.5	-15			-64	-4	-57.5
2	-80	-32.5	-90	-37.5	-97.5	-85	-96	-52	-87.5
3		-37.5	-95	-87.5					-100
4		-50			-100	-100			-100
5		-77.5							
6		-57.5							

## EXPLANATION OF THE TABLE

The data from each set are summarized in a short column. Reading from the top downwards, the first line gives the initials of the subject, and the second line the distinguishing

<sup>1</sup> This set is merely the sum of sets 0-IV., 0-V. and 0-VI.

Subj.	SW	SW	SW	SW	SW	SW	SW	SW	SW
Set.	9-II.	9-I.	7-I.	7-II.	8-II.	7-III.	8-I.	8-IV.	8-III.
Mode.	Aud.	Aud.	Aud.	Aud.	Aud.	Aud.	Vis.	A.-V.	V.-A.
Type.	T-t	T-s	T-s	Rate	2's	4's	3's	3's	3's
Std.	232	232	435.5	435.5	435.5	435.5	435.5	435.5	435.5
No.	40	40	24	40	40	40	40	25	40
-7								80	
-6								68	
-5								36	
-4	77.5	85					77.5	24	
-3	62.5	82.5	87.5	100			-55	-24	
-2	50	32.5	58.3	80	92.5	95	52.5	-24	100
-1	2.5	35	12.5	-10	30	37.5	30	-60	92.5
0	27.5	7.5	37.5	22.5	47.5	35	12.5	68	60
+1	47.5	55	12.5	80	90	97.5	37.5	92	12.5
+2	35	35	20.8	92.5	100	100	55		42.5
+3	77.5	70	62.5	100			72.5		95
+4	72.5	65	66.6						
+5	96.4		83.3						
+6	96.4		87.5						

Subj	GD	GD	GD	GD	GD	GD			
Set.	10-I.	10-II.	11-III.	11-I.	11-II.	10-III.			
Mode.	Aud.	Aud	Vis.	Aud.	Aud.	Electr.			
Type.	6's	4's	4's	S-W, 4's	W-S, 4's	Rate			
Std.	232	232	232	232	232	232			
No.	32	40	16	40	40	25			
-3						32			
-2	87.5	82.5	80	67.5	90				
-1	15.6	32.5	40	22.5	80				
0	6.2	17.5	20	25	27.5	8			
+1	56.2	77.5	13.3	65	35				
+2	81.2	92.5	26.6	95	85				
+3			20			28			
+4			6						
+5			24						
+6			18						
+7			48						

number of the set. The numbers correspond with those of the graphs in Figs. 1-11. For example, 3-III. indicates that the data of that column are those from which Graph III. of Fig. 3 is based. Sets not represented in the figures are given the number 0. The third line gives the mode of the experiment. 'Aud.,' 'Vis.,' and 'Electr.' indicate that the stimuli were auditory, visual, or electrical, respectively. 'A.-V.' indicates that the standard was auditory, and the variable visual. 'V.-A.' indicates the converse (visual standard and auditory variable). The fourth line gives the type of the experiments. 'T-s' (for Time, single) indicates

that each judgment was the comparison of the interval between a standard pair of stimuli, with the interval between a variable pair. 'T-t' (Time, triple), indicates a similar comparison of a standard repeated three times with a thrice repeated variable. 'Rate' indicates the comparison of two series of stimuli (determination whether the variable is faster, or slower, or equal to, the standard), without rhythmic grouping. '2's,' '3's,' and '4's' indicate rate judgments when the impressions are grouped rhythmically in twos, threes, and fours, respectively.

The fifth line gives the standard interval or rate, in terms of the interval between the beginning of one stimulus and the beginning of the next, expressed in sigma. The sixth line gives the number of judgments on each of the variables of the set: in set 1-I., for example, there were 25 judgments on each of nine variables—175 in all.

The variables are indicated by the numbers at the left of each of the lines after the sixth. '0' indicates the variable equal to the standard, '- 1' indicates the next shorter (or faster) variable, '1' the next longer (or slower) value, and so on. To find the actual value of the variable on which the judgments in any line are based, multiply the number at the left of the line by 2.75 for the 232 standard, and by 7.4 for the 435.5 standard, and add the result to the standard.

In the case of each subject, the sets are inserted in the table in the order in which they were taken. In nearly every case one set was completed before the next set was commenced (the subject being given a period of practice under the new conditions, without record of judgments, as described in §2 above). The exceptions are sets 0-IV. to 0-VII., 2-I. and 2-II., and 11-I. and 11-II. All four of the 0-sets were carried on concurrently, as were both of the 2-sets and both of the 11-sets, a certain portion of each of several successive periods being given up to each of the four (or two) sets.

#### 4. INTROSPECTION AND BEHAVIOR OF THE SUBJECTS

Three of the subjects were freshmen, one (*GW*) a graduate student, and the fifth (*MW*) a woman. Very little intro-



spection was required from them, except on the points of their instructions and attention.

*GW* inclined to the opinion that the differences were largely 'qualitative.' In some cases they seemed purely qualitative, and in others (in certain time-series) partly intensive. In the latter case, the essential difference between two time intervals seemed very like the difference between two 'physical strains,' of which one was more severe. (This subject was at the time reading Bergson's 'Time and Free-will').

All subjects based the rate judgments on the whole of the standard series and the first part of the variable series. In most cases the judgment was made before the end of the variable. Toward the end of the investigation, the men were requested to telegraph their judgments as soon as made, instead of waiting for the end of the series. Under these conditions, the signals were in most cases received by the experimenter about a second before the end of the variable. Very seldom was a judgment signaled after the end of the series.

*CW* and *SW* felt more certain of the judgments of 'faster' than of those of 'slower.' *SW*, on the other hand, found it easier to detect the slower than to detect the faster rates; or, as he expressed it, to detect the *ritard* than the *accelerando*.

All subjects declared that the grouping in twos, threes, and fours was easy and natural; in threes and fours perhaps most easy, in sixes slightly less so. In sets 2-I., 2-II., 8-III., 8-IV., 8-I., 11-I., 11-II., and 11-III., the subjects were instructed to group in the way which seemed to them most natural.

*SW* grouped at times by 'counting mentally,' and was conscious of no movements, even of the vocal organs. At other times; he did not seem to count; could not tell what formed the groups, although they were distinct. *CW* also counted the groups. At first there was a tendency to move foot at each accent, although he did not think the foot actually moved. This persisted only for a day or two. Then he felt a slight tongue movement, or impulse to movement, in counting; this also soon disappeared, and no movements

were detected or impulses during the greater part of the work with grouped series. *GD* was uncertain in his introspection. He was not sure that he counted, in many cases, although he did count sometimes. He thought there was no muscular activity connected with the counting or grouping.

In the visual series, *GW* and *GD* gazed at a fixation point approximately 25 degrees to the left of the surface illuminated by the stimulus. This fixation mark was a square of white paper on a black ground, and was dimly visible in the partially darkened room. *SW* gazed part of the time at the stimulus area, and part of the time to the left. Several series were taken on *CW*, but the experiment was discontinued because he found it very disagreeable, and judgments difficult. He said that a continuance of the experiment would be positively insupportable. The other men found the visual experiments less trying than the auditory, and the two who tried grouping found the groups as distinct and natural as with the auditory stimulus.

*CW* noticed that the visual series 'speeded up,' *i. e.*, apparently went faster towards the middle and end of the series than at the beginning. *SW* at first noticed that the series sometimes increased in apparent rate, and sometimes decreased, but later did not notice these changes. *GW* found very definitely that the apparent rate of a series might change in either direction, if the eye moved, but that when the eye remained motionless (as nearly as he could tell), that the change did not occur.

*GW* noticed that the slower of two visual series seemed to have more contrast; that is, that the change from the illumination during the flash to the dark interval following, and *vice versa*, was greater. He was inclined to think that his judgments were based on this feature of the series, but was not certain on this point.

## 5. REMARKS ON THE RESULTS OF THE EXPERIMENT

An examination of the figures shows that the sensibility for rate differences is more acute than the sensibility for time differences, at least under the conditions of the experiment.

This fact is expressed in the greater steepness and greater regularity of the graphs for rate discrimination, as compared with the graphs for time discrimination. Not only does a certain difference between standard and variable give a larger percentage of discriminations in the rate series than in the time series, for all ranges of difference up to those which give 100 per cent. of discrimination in the time series, but also the judgments of rate difference are so regular that sets of from twenty to forty judgments give a very smooth graph (showing that the number of judgments to a set were practically sufficient), whereas even where the time-set ran over forty judgments to a variable, the graph is irregular, not to say erratic.

These differences favor the supposition that the rate-judgment is not essentially a judgment of the interval between stimulations. It can hardly be claimed that the advantage in the rate-judgments was due to the repetition of the time-interval providing a better basis for the time-judgment, for the series with triple presentation of standard and variable (3-II. and 9-II.) do not give results differing essentially from the results in the corresponding sets with but one presentation (3-I., 9-I.).

We have implied that the irregularity in the graphs for the time-experiments indicates merely an insufficiency of judgments in these experiments. This may not be true. The curious way in which graphs I. and II. of Fig. 3, and I. and II. of Fig. 9 correspond in the irregularity of their lower courses, is certainly remarkable. Graphs II. and III. of Fig. 1 suggest the same sort of correspondence, although the kink in III. is placed higher than the kink in II. This may be a practice effect, as set III. was taken after set II., and under the same conditions, but it seems hardly likely. In this case, averaging the two sets obliterates the peculiarities in question. In the other two cases, averaging the sets does not produce such an effect. In Fig. 7, Graphs I. and II. have a curious correspondence of irregularities, but it is not well marked.

Rhythmic grouping of the impressions gave in every case



a more acute difference sensibility, as compared with the corresponding cases in which there was no grouping (compare III. and IV. of Fig. 3; II. and III. of Fig. 4; II. and III. of Fig. 7). Although the increase in acuteness is slight, the fact that it uniformly occurs seems important, since it is out of harmony with the supposition that rhythm perception is essentially a matter of time perception, and fits in with the theories which explain rhythm as a periodicity in consciousness. We may well suppose that if the consciousness-period (whether the succession of specious presents or of attention-waves) be adapted to one rate of stimulation (the standard), a second rate (the variable) will not fit in with that period, and if it differs sufficiently from the standard the incongruity may be perceived, independently of any difference in the perceptible rates of succession of the single impressions, and of any difference in the perceptible time-lengths of the groups.

The approximate threshold values in the results of the auditory experiments are quite low. In every case the upper and lower threshold are both included within the limits of three steps in the scale of variables. If the upper threshold is between - 1 and - 2, the lower is between 0 and 1; if the lower is between 1 and 2, the upper is between 0 and - 1.

One step in the scale is 1.18 per cent. of the 232 standard, and 1.67 per cent. of the 435.5 standard. The difference thresholds, therefore, for the 232 standard are all *below 2 per cent.*, and for the 435.5 standard *below 3 per cent.* The total range of no discrimination for the 232 standard is under 3 per cent., and for the 435.5 standard under 4.5 per cent.

The constant errors are slight in the auditory experiments, and are in both directions; we should perhaps consider them as mere irregularities in the position of the absolute no-discrimination than as typical constant errors. The fact which we have stated above, that the thresholds are not symmetri-

<sup>1</sup> It is quite possible that in many cases objective time-intervals may be compared by noticing the congruity or the incongruity of one interval with a specious present or a succession of specious presents, and that this may be the most accurate method of comparison in the cases to which it is applicable. The work of the Leipzig experimenters on the time-problem takes on new interest in this connection. Such comparison, although of time-intervals, would not be time-estimation; that is, it would not be the comparison of time or duration contents.

cally placed with regard to the point of actual equality is not so significant as the numbers (0 and 1, 1 and 2) would seem to indicate, as we have taken no account of the closeness of the threshold to one or the other of the steps between which it lies. A scrutiny of the graphs, with regard to the abscissæ of their intersections with the *X*-axis and also with regard to their general courses strengthens the impression that the factor of constant error is insignificant in the sets under consideration.

Several of the graphs, notably those of the time experiments, show indications of a factor which, under the proper circumstances (especially, under the conditions of the method of minimal change), might simulate a constant error. The angle which the upper part of the general course of one of these graphs makes with the *Y*-axis is much sharper than the corresponding angle for lower course. This peculiarity is especially noticeable in 3-II., 5-I., and 7-I., and is grotesquely exaggerated in the graph of one of the visual sets (II-III.). We may reasonably suspect that the comparison of a standard with a succeeding longer (or slower) variable is more difficult than the comparison of the same standard with a shorter (or faster) variable.

The results of the visual experiments show that the rate difference sensibility is less acute for the visual conditions than for the auditory. The single electrical series, with only three variables, does not offer sufficient basis for any inference.

The two pairs of sets with standard of one mode and variable of the other (8-II. and 8-III., 5-IV. and 5-V.) give results which are unmistakable. The visual series seem slower than auditory series of the same actual rates. This fact should be considered in connection with the results of the auditory series with two intensities (2-I. and 2-II., 6-I. and 6-II., II-I. and II-II.).

In two cases, the series with loud sounds seemed to be faster in rate than the series with weak sounds. In the third case there was practically no difference in the apparent rates with the two intensities. This is what we might expect in ungrouped series, from the converse effect of rate on apparent

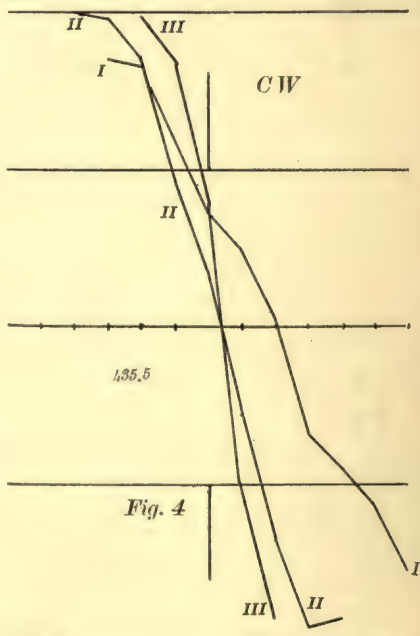
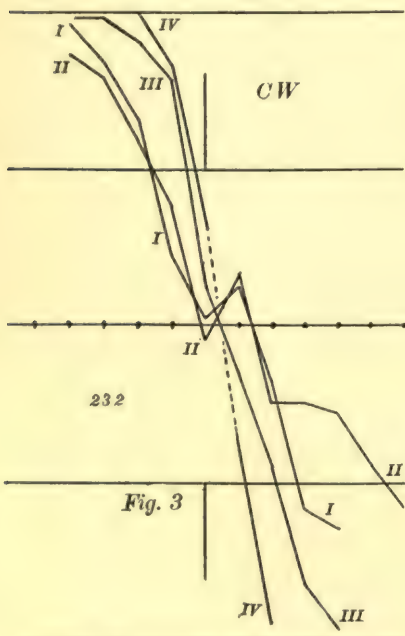
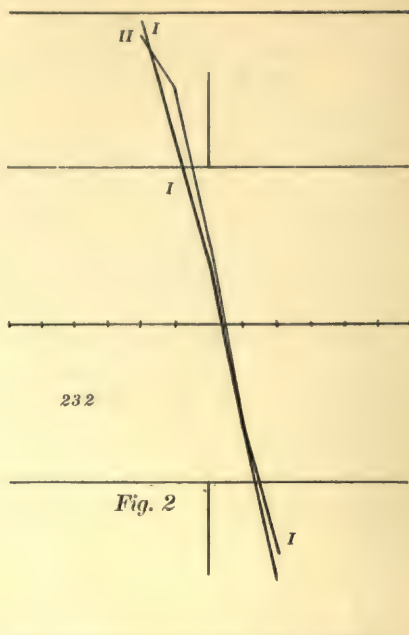
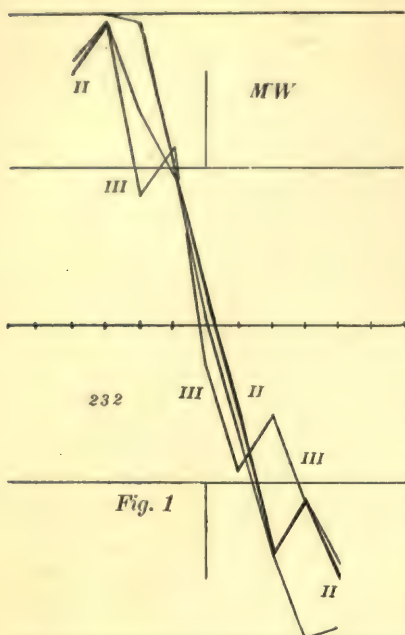
intensity. We might also expect that grouping the stimulation impressions should in some cases neutralize the effect of the intensity on the apparent rate. The explanation usually given for the effect of rate on apparent intensity, however, can not be converted into an explanation of these results, unless we are willing to say that the difference in apparent rates of visual and auditory series rests on entirely another basis than that on which rests the difference in the cases of two series of different intensity. If we can admit any inter-modal comparison of sensation intensities, the visual impressions with which I worked were much more intense than were the auditory impressions, and the visual impressions certainly ran together temporally to far greater extent than did the auditory impressions.

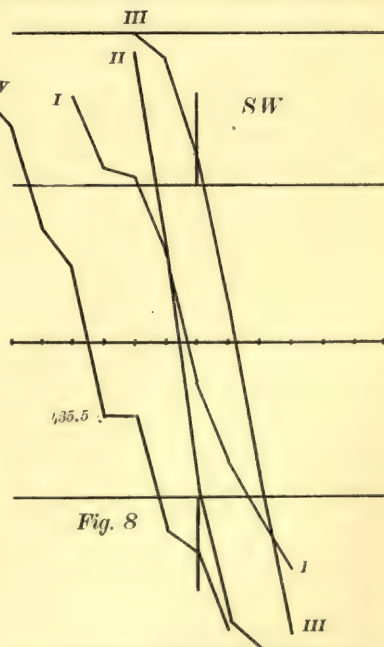
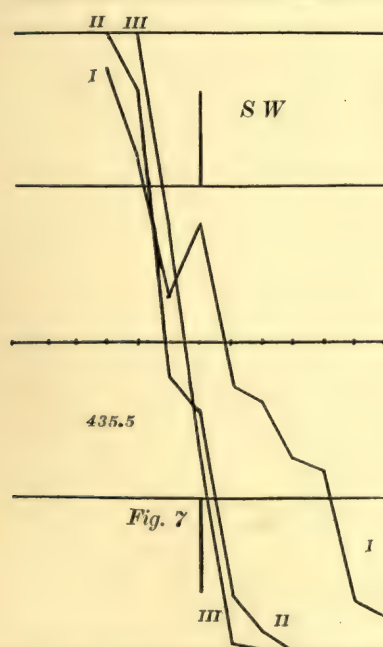
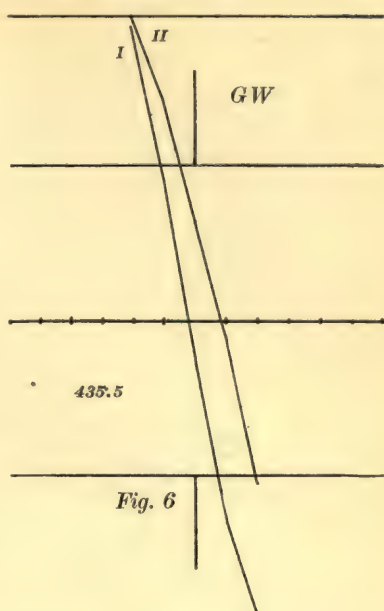
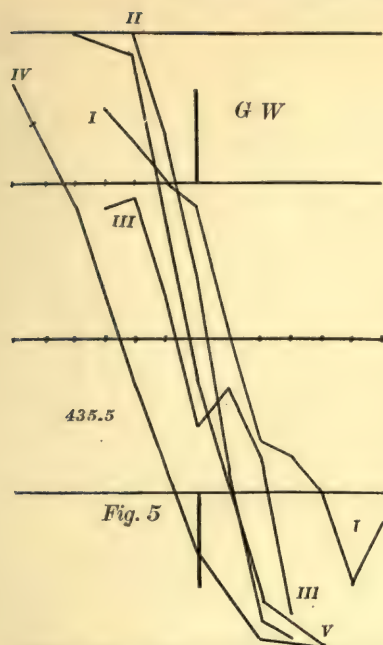
The results of the bi-modal series may be harmonized with those of the bi-intensive series on the basis of strain sensations. The visual series provoked eye-strain in the (successful or unsuccessful) attempt to avoid eye-movements, whereas the perceptible strains in the auditory series were less intense. There was, however, more strain (possibly of the tympanic muscles, but probably of the neck and thoracic muscles) in attending to the weaker auditory impressions than in attending to the stronger. While we may not be willing to definitely espouse this theoretical interpretation of the results, it is nevertheless attractive and promising. Strain sensations are undoubtedly an important (although not the sole) factor in time content, and while it is probable that the estimation of rate rests on grounds which are not identical with the time content, it is also probable that the time factor may modify or even override the direct rate judgment. The time and the rate factors may both be important even when the judgment is based largely on the rhythmic factor or factors. It is possible, for example, that the consciousness-period carried over from one series to the succeeding one may be modified by factors in the second series which affect the rate or time judgment directly and do not affect the rhythmic impression directly.

The rate discrimination is approximately as acute with

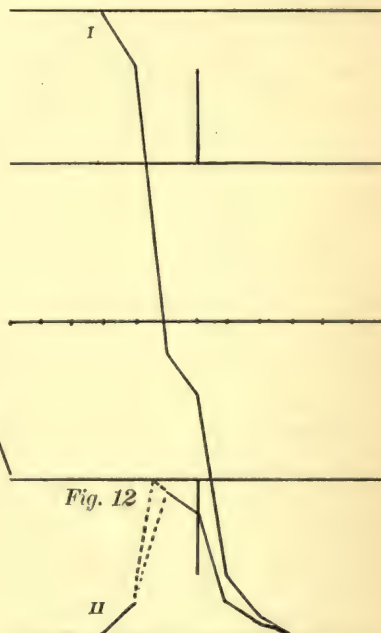
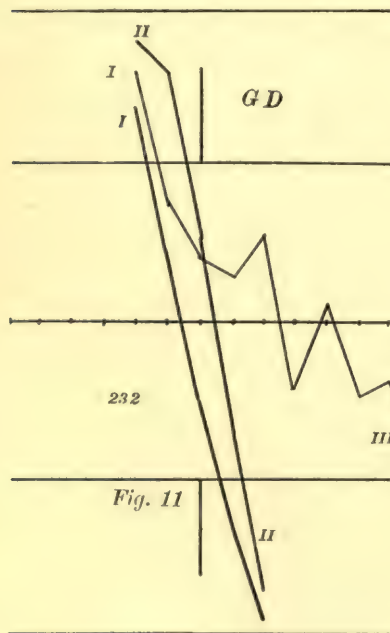
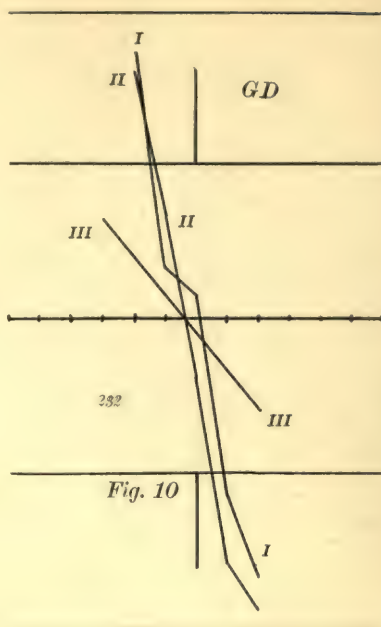
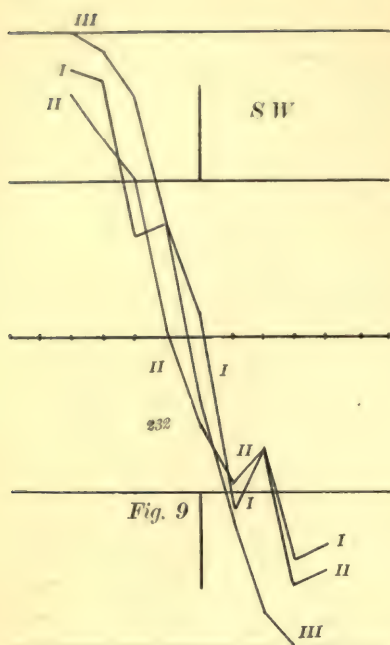


the telephone noise, in spite of its irregularities, as with the vastly more uniform sounder click. This is visibly apparent from the graphs: compare the slope of 11-I. and 11-II. with 10-II.; 6-I. and 6-II. with 5-II.; 2-I. and 2-II. with 3-IV. Apparently a large amount of irregularity in the stimuli-series is inconsequential, provided the series is based on a distinct regularity. It will be interesting to find out if this principle can be confirmed and extended in regard to rhythmic series in which each group is marked by an impression whose period from group to group is regular, while the periods of the other impressions are irregularly varied.









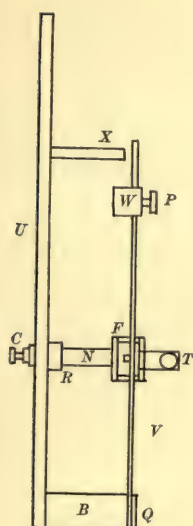


Fig. 13

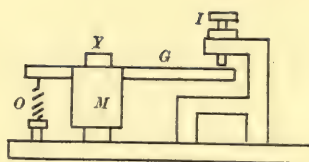


Fig. 15

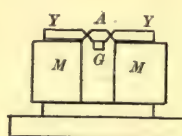


Fig. 16

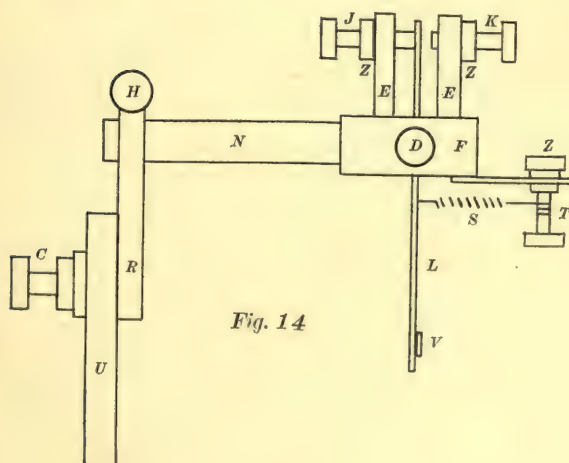


Fig. 14

## SOME NOVEL EXPERIENCES<sup>1</sup>

BY H. A. CARR

### I. POSITIVE AFTER-IMAGES OF MOTION

The following curious phenomenon was experienced by Professor Watson, while aiding the writer in a dark room experiment in this laboratory during the summer of 1906. After serving as a subject in a test involving considerable eye fatigue, Professor Watson was engaged in carefully and steadily observing one of the writer's eyes throughout several periods of five to six minutes duration each. The room was pitch dark with the exception that the observed eye was illumined by a miniature electric flashlight. The light was of weak intensity, but all of its rays were converged upon the eye.

After one of these observations, the flashlight was turned off for a period of rest. Shortly afterwards there developed in the darkness an extremely vivid and realistic positive after-image of the eye—the dark iris and pupil, the white sclerotic, and the surrounding lids—all appearing in a faint halo of white light, the positive after-image of the illumination. All of the minor details of coloring and marking came out distinctly. The image was hardly ghostlike in appearance for it appeared too substantial in character; rather it looked alive, and it so dominated consciousness that it could not be destroyed by eye closure, eye movement, nor by diversion of the attention. So uncanny was its life-like reality that the lights were finally turned on in the room in order to escape its persistent presence. The phenomenon had persisted from two to three minutes. Just before the lights were turned on, an added tinge of reality was produced by the occurrence of a wink of the reflex type. Evidently this wink was a positive after-image of the involuntary blinkings occurring during the prolonged experiment. It was decided to repeat the phe-

<sup>1</sup> From the Psychological Laboratory of the University of Chicago.



nomenon at other times and note more carefully the conditioning circumstances.

A similar after-image of the eye was obtained in three tests and five cases of winking were noted. Three of these cases occurred during the duration of one image. Moreover, in one test the image of the eye was observed to rotate several times. The amount of rotation was small but distinctly noticeable. These movements were exact replicas of the small reflex rotations occurring during the observation.

It was found necessary to induce a general fatigue of the observer's eyes before the experiment was successful. A more or less definite period of exposure to the stimulation seemed to be necessary in order to obtain the best results. This period was three to four minutes in length. The after-image did not develop until two or three minutes after the cessation of the stimulation. The image generally existed for a period of five to six minutes. Consequently the after-image of motion might occur eight to ten minutes after the original perception of the movement. No negative after-image of the eye was observed either before or after the positive phenomenon. The positive image fluctuated slightly in intensity. These after-images of motion occurred while the eyes were either open or closed. They were not synchronous with the involuntary blinkings nor with the inhibited tendencies to wink on the part of the experiencing subject, at least so far as he was cognizant of these tendencies. Apparently their occurrence was as given and independent of the subject as any objective phenomenon could be.

Professor Watson has had considerable practice in the observation of after-images and is, apparently, more than ordinarily sensitive to the phenomenon. The writer repeated the experiment but was unable to obtain even a positive image of the eye under these conditions of stimulation.

The novel feature in this phenomenon is the presence of motion in the after-image—the blinkings and rotations on the part of the phantom eye. Positive after-images of motion are common and well-known experiences, but our phenomenon presents a new aspect.

A positive after-image of a moving object successfully fixated during its movement gives the sense of motion. This movement of the image is generally supposed to be due to the momentary persistence of the pursuit movement of the eye-ball. The after image of motion also occurs when the eye is stationary during the stimulation. This result is generally explained as due to the fading of the positive after-image streak which does not disappear simultaneously throughout its length. A third case is obtained by fixating a light which by a rapid rotation induces a continuous band of light in which is perceived the motion of the more intense portion. The after-image is a plain band which rotates like a wheel. The band does not present any intense part corresponding to the light which moves in reference to the remaining portion as in the original experience. The circular band is relatively homogeneous and rotates as a whole like a wheel without any of its parts moving in reference to each other. The explanation of this phenomenon is difficult.

Our phenomenon differs from those mentioned inasmuch as it presents a movement within the image itself—a positional change of its parts in reference to each other. Rivalry seems to be the best explanation. In the winking the images of the lid and cornea rhythmically supplant each other. The rotation involves not only the struggle of adjacent contents for dominance but rhythmical changes of size, form, and brightness in certain parts. It is rather surprising that these changes should occur so harmoniously several times in succession as to induce such clear-cut definite effects. It is probable that not all of the changes which one can analytically detect in a rotating eye actually occurred in the after-image; it is more probable that only a few striking sensory changes were present and that the rest was a matter of suggestion.

## 2. AN ILLUSION OF DOUBLE AWAKENING

The following peculiar experience has been reported by a friend of my family. The experience consists of three successive stages. The first stage is a dream in which occurs the usual illusion of being awake. In the second stage the

subject regards the first stage as a dream, but still is under illusion that she is awake at the time. Inasmuch as the transition between the two stages is continuous, although abrupt, the subject is under the impression that she was awakened at this transition. As a matter of fact the second stage subsequently proves to be a dream. The third stage proved to be the normal awakened state in which the subject is aware of the two previous stages and her attitudes toward them. We have, then, two dream stages succeeded by the usual awakened consciousness, and both of the transition points are characterized by the sense of awakening, and consequently the subject experiences the phenomenon of two successive awakenings.

This phenomenon was first reported to the writer while the subject was a member of one of my classes in psychology. The subject was requested to note all the details of any subsequent repetitions of this experience. After an interval of two years it was reported that in the meantime the phenomenon had occurred a number of times in her dreams. The phenomenon is not connected with any special kind of dream. In fact, the actual content of the dreams varied. Owing to the frequency and novelty of the experience and my request for a detailed description, the subject feels sure of the main features of the phenomenon as described. Two additional facts were reported: (1) The sense of being awake during the first stage was not overtly and explicitly present to consciousness. The reality of the experiences was accepted in a tacit, matter of fact way. The awakened attitude in the second stage was, however, explicitly and overtly present, and it was accompanied by a high degree of certainty and assurance. (2) The transition from the first to the second stage was invariably accompanied by a noticeable increase in the apparent illumination of the visual world. Objects stood out with great distinctness, and visual space was more visible and transparent. The difference was similar to that of a room at dusk and under a medium degree of artificial illumination. The luminosity of the second stage was, so far as the subject could recall, always of an artificial character.



Dreams are characterized by one of three attitudes. (1) The subject is often genuinely aware that the dream is a dream. This dream attitude is overtly present to consciousness but with varying degrees of certainty and assurance. (2) The subject may experience the illusory sense of being awake, and this attitude with varying degrees of certainty is explicitly present. (3) The tacit and implicit acceptance of the reality of dream experiences is probably the normal and most frequent attitude. This awakened attitude, however, is not present in an overt and explicit form. While we do attribute the awakened attitude to dreams of this type, yet this attitude probably is read back into the dream from considerations derived from subsequent normal experiences. The attitude may be said to be present during the dream only in a tacit and implicit manner.

The phenomenon which we have described possesses but one novel feature, viz., the illusion of awakening, or the change of attitude at the first transition point. The case must be distinguished from a theoretically possible one in which there is a change from a dream to an awakened attitude, or vice versa, but in which the new attitude refers to the entire dream both preceding and subsequent to the transition point. In the case described the new dream attitude applies only to the first stage preceding the transition. The explanation of the phenomenon probably concerns the apparent increase of the luminosity of visual space. The contrast due to this abrupt change overtly arouses the awakened attitude with its high degree of assurance and as a consequence there is induced the retrospective change of belief as to the character of the preceding experiences. The phenomenon is exactly similar to normal awakening.

It may be suggested that possibly the two transitions represent two real stages in the awakening process. In this sense it is hardly proper to term the phenomenon an 'illusion' of double awakening. Awakening may occur suddenly and abruptly, or it may occur so gradually that the person is unable to designate even within wide limits the point where the dream merged into the awakened state. If the awakening

may be gradual or abrupt, there is no *a priori* reason why it may not occur with two abrupt transitions.

### 3. RECALL THROUGH SIMILARITY

A young lady of my acquaintance suffers many frequent memory lapses for immediately preceding experiences. For example during a public musical performance, she may 'come to' with a start at the finish with a keen consciousness that she can not remember anything that has occurred in the meantime. The lapse however does not interfere with her playing. These lapses generally occur during fits of mental absorption to which she is addicted. My interest in the phenomenon was directed toward the question whether these experiences were ever subsequently reinstated and to the conditions and mechanism of their recall. This account refers to one such case of reinstatement by similarity or partial identity. She was walking home one afternoon accompanied by a man who was carrying a package. Upon entering the house, her mother inquired the name of her companion and was told that she had walked home alone. Since both were familiar with these memory lapses, the incident was an occasion for much merriment in the family, and the subject attempted to recall the circumstances without success. On the following day a delivered package attracted her attention, striking her with an unwonted sense of familiarity. While observing the package, the forgotten experiences were fully reinstated, and she was aware in an anticipatory manner that the package was the bond of connection, inasmuch as her companion of the day before had carried a package whose size, shape and general appearance were strikingly similar to that before her.

# THE INFLUENCE OF CAFFEIN ON THE SPEED AND QUALITY OF PERFORMANCE IN TYPEWRITING

BY H. L. HOLLINGWORTH

*Columbia University*

The greater part of the previous work on the influence of drugs has been directed toward the study of relatively simple mental and motor processes, such as reaction times, free and controlled associations, reading, adding, hitting at dots, and especially the production of ergograms. In the present experiment, in addition to the investigation of a series of similarly simple processes to be reported elsewhere, an attempt was made to measure the influence of caffein alkaloid on a more complicated process,—that of performance in typewriting.

Subject No. 2, a woman of 38 years, already fairly proficient in typewriting by the touch method, did not take part in the tests through which the various squads of the larger experiment were put. Instead she made systematic records of her skill in typewriting throughout the four weeks of the experiment. Ruskin's 'Sesame and Lilies' was chosen as the material to be copied, since it is fairly uniform in character and interest throughout and was unfamiliar to the subject. The pages of the edition used contained 27 lines, the lines containing on the average 35 characters (letters and punctuation marks). The pages were placed in a random order on an improvised holder, directly over the machine and on a level with the writer's eyes. Care was taken to keep the lighting conditions as constant as possible and the amount of disturbance through the day at a minimum. The subject corrected all mistakes noticed at the time they were made, and record was made of (1) the time taken to write the standard amount, (2) the number of corrected errors, and (3) the number of errors passing undetected. The time record was



kept by the subject herself, by means of a stop-watch. The errors were counted, after the close of the experiment, by a second person<sup>1</sup> and checked up by a third.

During the first 27 days of the experiment the standard amount was 3 pages. This amount was written 7 times daily, the hours being at 8:00, 9:00, 10:00, 11:00, 2:00, 3:10 and 5:30, in order to distribute the trials as much as possible over the entire day. During the first week only sugar doses were taken, the object being to reach a practice level and to secure perfect adaptation to the conditions of the experiment before the caffein was administered. After the first week caffein alkaloid doses were given, in capsule form, on alternate days, the subject being in no case able to distinguish between the caffein days and the control days. This arrangement gave two days for each of the 1, 2, 3, 4 and 6 grain doses employed. The doses were given in increasing amounts, and in all cases just after the first trial for the day had been made, this time being about 8:30 A.M. When caffein is taken in capsule form its effect does not begin until about one hour after taking. Consequently, besides comparing the absolute amount of work done on caffein days with the amount done on control days, the first two trials of each day may be used as a normal performance for that day and the ratio of the five later trials to this normal computed for both kinds of days.

On the remaining 3 days (the intensive experiment) the subject came to the laboratory daily at 10:00 A.M. and wrote 2 pages each half hour (excepting short intermissions for lunch and dinner) until 9:15 P.M., thus making 19 trials each day. On the first of these three days 3 grains of caffein were taken at 3:15 P.M., just before beginning the 10th trial. On the second day a control capsule was taken at the same hour and on the third day a 6-grain dose of caffein. During these days there was absolutely no evidence of practice effect, the subject having reached her level some time before the intensive experiment began. It should be stated that when the book had been copied through once its pages were shuffled again and rewritten in random order.

<sup>1</sup> For assistance in this laborious part of the study the writer is greatly indebted to Miss Agnes M. Denike, A.B., Barnard, '11.

Rivers has made some use of the typewriting test in his work on the effects of caffeine and alcohol by inserting periods of writing between the successive performances on the ergograph. In the case of alcohol neither the speed nor the accuracy of the writing seemed to be affected. "There is certainly no indication of any favorable action of the alcohol" (p. 96). "The errors in typewriting fall into two classes—those which escape notice and those which are noticed and corrected. . . . It will be seen that the latter are not very numerous, and so constant in number that they give not the slightest indication of an alcohol-effect. The uncorrected errors occur more frequently, and show an unmistakable tendency to increase with the rapidity of the work, being most numerous in the second interval of the fifth day, when the amount of work reached its maximum. When this increase with rapidity of work is taken into account, there is no definite indication of any alcohol-effect" (pp. 97-8).

There is, however, a striking discrepancy between these statements of Rivers and the table (p. 96) on which he bases them. The table referred to is given complete below and the discrepancy pointed out because of its bearing on certain results of the present experiment.

(TABLE III)

RIVERS, THE INFLUENCE OF ALCOHOL AND OTHER DRUGS ON FATIGUE, P. 96

	May 17, No Dose	May 19, Control	May 21, 40 c.c.	May 23, 20 c.c.	May 25, No Dose	May 27, 20 c.c.	May 29, Control	May 31, Control
1st interval:								
Quantity of work.	832	824	841	884	883	847	871	902
Corrected errors.	47	56	86	80	89	74	86	94
Uncorrected errors.	26	30	38	26	39	27	21	31
2d interval:								
Quantity of work.	797	842	805	884	956	897	885	904
Corrected errors.	86	71	80	92	140	99	107	127
Uncorrected errors.	45	46	31	26	42	36	44	19

Contrary to the statements quoted in the preceding paragraph, the *corrected* errors are without exception much more numerous than the *uncorrected*. This, it will later be seen, was also the case in the present experiment. The absolute

numerical proportion between the two types of errors is of course immaterial and even their relative numbers would depend chiefly on the attitude of the subject toward the question of corrections.

In Rivers' experiments with caffein .3 gram of caffein citrate, equivalent in strength to about 2.5-3 grains of the alkaloid, was taken morning and evening for six days, and on mornings only for another 6 days, adequate control doses being employed (gentian and citric acid). The dose was taken 10 minutes before the work began. With respect to speed, this experiment showed "the distinct superiority of the caffein days" (p. 45). The number of mistakes was also determined and "here it came out quite definitely that the drug was without influence."

Of the three most available methods of presenting the results of the present experiment only two show clear results. One might on the one hand map out the efficiency curve for the various days, thus indicating, on each day, the course of the performance before and after taking the dose. But diurnal variations arising from other causes obscure the relatively slight influence of the drug from this point of view. A more satisfactory way is to compare the total amount of work done on the caffein days, after the dose has been taken with the similar records afforded by the control days, thus indicating the general capacity for work rather than the diurnal course of efficiency. Or instead of the total amount of work done we may use to advantage the ratio of this amount to the normal work of the respective days, the normal consisting of the work done before the dose was taken.

From the point of view of speed of performance both of these latter methods show clearly that doses of 1-3 grains of caffein alkaloid are stimulating in their effect while larger doses (4-6 grains) produce retardation. Plate I. records the total time taken for the 6 trials after the capsule was taken, for each of the 19 days of the first series of tests, beginning with the last day before the caffein doses commenced. The broken line follows the records for the control days and the solid line that for the caffein days, the time being recorded in minutes.



The 1 and 2 gr. doses show indication of decided stimulation, but at the 3 gr. dose the curves cross, the larger doses of caffeine yielding longer times than those of the control days. Presented in this manner, however, the stimulation is somewhat obscured by the practice effect shown by the curves as a whole.

In order to eliminate this factor of practice to a greater

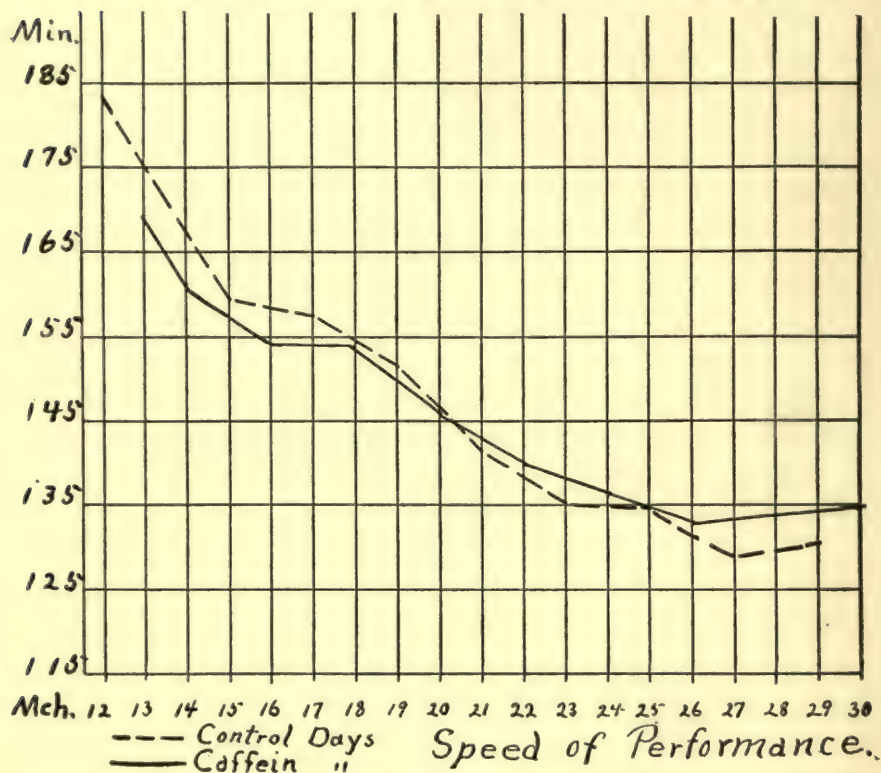


FIG. 1.

degree and to allow for daily variations of an irrelevant sort, I have computed the ratio of the average performance after the dose to the normal performance of each day, this normal being secured by averaging the first two morning trials. These ratios give the curves of Plate II., in which again the broken line represents the ratios for the control days and the solid line those for the caffeine days. The effect suggested by

the curves of Plate I. is here very clearly repeated, except that the retardation does not appear until the 4-grain dose is reached. The curve for the control days is practically a horizontal line, showing uniform maintenance of the normal throughout the control days. But the solid line shows improvement over the daily normal after doses of 1-3 grains and retardation below the daily normal after larger doses.

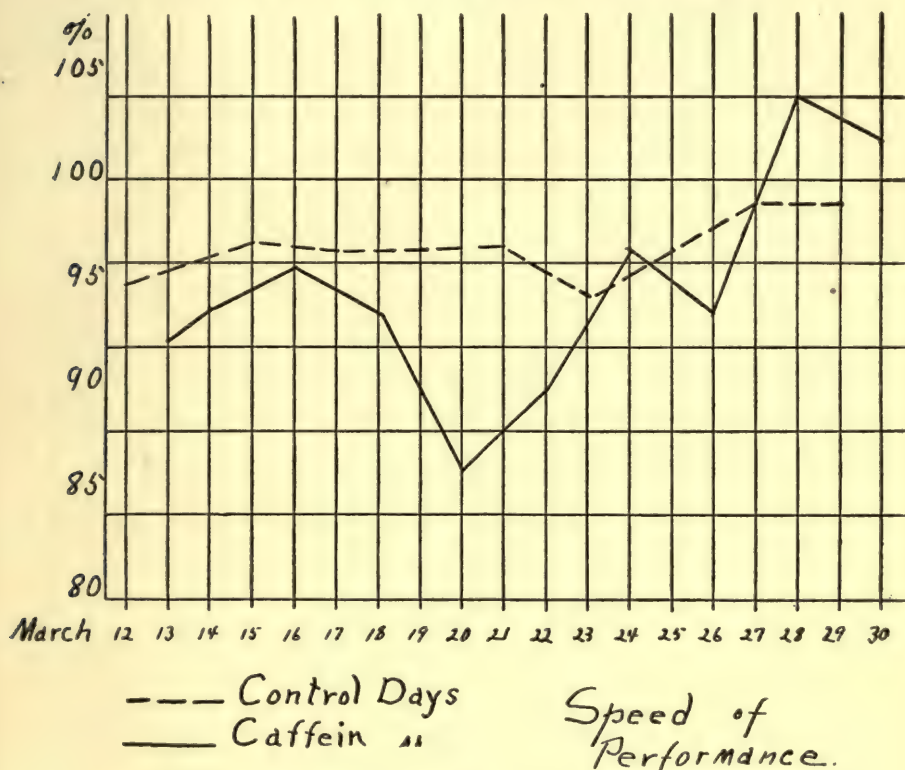


FIG. 2.

Table I. shows the final averages of the daily totals for the last five trials of all the days. The control days and all the caffein days, regardless of the size of the dose, are averaged, and the time, the number of corrected errors, the number of uncorrected errors and the total errors recorded in separate columns.

TABLE I

Averages for	Time	Errors Corrected	M.V.	Errors Uncorrected	M.V.	Total Errors
Control days...	146.7	208	7	73	14	281
Caffein days...	145.7	201	11	66	13	267

The average times balance, on account of the compensating effects of the small and large doses. The averages for both kinds of errors are smaller on caffein days than on control days, but the mean variations are fairly large and the difference is quite within the range of the probable error. Nevertheless the fact that it is a genuine difference is apparently borne out by the results of the intensive experiments which follow. But the difference is so small that curves corresponding to those for the times fail to show anything clearly.

Table II. gives the results of the three-day intensive experiment, in which 19 trials were made daily, the dose being taken at the time of the 10th or median trial. The table gives the totals for the 9 trials before the dose, comparing these with the totals for the 9 trials following it.

TABLE II

## INTENSIVE EXPERIMENT

		First Nine Trials	Last Nine Trials	Difference	Per Cent. Loss or Gain
Time.	Control,	128.6	132.1	+ 3.5	+.027
	3 grains,	130.1	129.5	- .6	-.005
	6 grains,	131.5	131.3	- .2	-.002
Corrected errors.	Control,	189	176	- 13	-.068
	3 grains,	216	156	- 60	-.277
	6 grains,	273	170	- 103	-.377
Uncorrected errors.	Control,	35	49	+ 14	+.400
	3 grains,	42	35	- 7	-.167
	6 grains,	59	34	- 25	-.423
Total errors.	Control,	224	225	+ 1	+.005
	3 grains,	258	191	- 67	-.259
	6 grains,	332	204	- 128	-.385

These figures confirm the previously drawn conclusions. With respect to speed of performance the slight fatigue present on control days gives place to very slight stimula-



tion on caffein days. The actual difference in time is, however, so slight as to be, in this case, scarcely worth mentioning. And this is what we should expect when the 3- and 6-grain doses are taken. But the difference in the number of errors of both kinds is very great. On control days the corrected errors are slightly less after the dose than before it ( $-6.8$  per cent.). But after the 3-grain dose they decrease much more decidedly ( $-27.7$ ) and after the 6 grains still more so ( $-37.7$  per cent.). The uncorrected errors are greater for the last 9 trials on control days, clearly less after 3 grains of caffein ( $-16.7$  per cent.) and strikingly less after 6 grains ( $-42.3$  per cent.). Compared with the first 9 trials the total errors for the last 9 trials are greater for the control day (less for the 3-grain dose, and still less for the maximum dose of caffein). Contrasted with Rivers' result for alcohol, the time and the errors grow less, simultaneously, and the superiority of the work, from the point of errors, which did not seem to be present in Rivers' tests of caffein, is clearly shown. But this superiority is seen only when the general capacity for work, over a considerable period of time, is examined. When the results are platted so as to show the course of the performance throughout the day, the curves are obscure.

#### SUMMARY

The speed of performance in typewriting is quickened by small doses of caffein alkaloid (1-3 grains) and retarded by larger doses (4-6 grains). The quality of the performance, as measured by the number of errors, both corrected and uncorrected, is superior, for the whole range of caffein doses (1-6 grains), to the quality yielded by the control days. Both types of errors seem to be influenced to about the same degree. The increase in speed is not gained at the expense of additional errors, but increased speed and decreased numbers of errors are simultaneously present.

## A NEW MEMORY APPARATUS

BY F. KUHLMANN

*Faribault, Minn.*

The great variety of memory investigations that has of recent years occupied the attention of psychologists has resulted also in a number of different kinds of memory apparatus. A few of these have but a limited use because of the special purpose for which they were designed. Some others have been constructed with the aim of meeting some one or two particular difficulties, and do not fulfill requirements in other directions. The apparatus that I shall describe is for the investigation of visual memory. It has some new features, but the chief aim has been to combine as many good points in one piece as was possible, and to produce an apparatus that would enable one to control as many as possible of the factors that enter a memory experiment, and which experience has taught require control. There are three parts to be described. (1) The exposing apparatus. (2) The apparatus for controlling the exposure interval. (3) The apparatus for controlling the retinal image.

The exposure apparatus consists essentially of a rectangular carrier frame running on four wooden rollers between two upright steel rods. This part is shown in Fig. 1, and the other accompanying cuts will help to make the description clear. Two of the rollers are supplied with small coiled springs that keep the tension and friction of the rollers against the uprights constant. The frame carries in its grooves a thin cardboard eight and a half by eighteen inches on which the memory material, if verbal, is typewritten in vertical columns, with a distance of two thirds of an inch between successive terms of a column. This is the distance of the line spacing of most typewriters. The carrier frame is held from dropping by the padded ends of two levers which come together like the two jaws of a pair of pinchers against the sides of a

vertical centerpiece of the frame, as seen in the cut, these levers being pulled together by two coiled springs. A pair of electro-magnets with proper attachments releases the tension of these springs when the circuit for the former is closed, and allows the carrier to drop till the circuit is broken again. The centerpiece of the carrier frame consists of alternating strips of brass and insulating material, a strip of either being two thirds of an inch high, and this is the distance the carrier will drop each time the electro-magnet circuit is closed. Figure 2 shows how this is accomplished. In Fig. 2, *a* represents the two mounted contact discs of the apparatus for controlling the exposure intervals and which is shown in Fig. 3. *b* is the centerpiece of the carrier, made up of the alternating strips of insulating material and brass, the latter being all connected with each other by a strip of metal in the back. *c* is the pair of electro-magnets. 1 and 2 are contact brushes, one set two thirds of an inch above the other, resting against the centerpiece. 3 and 4 are contact points which the teeth of the contact discs engage alternately as the two discs turn together on a common shaft. It will be seen that as the further contact disc closes the circuit at 3, and it being also closed at 1 resting on a brass strip, the carrier will drop until the brush at 1 slides onto the next and insulating strip, when the circuit is broken, allowing the springs to pull the levers together again. The brush at 2 will now be on a brass strip closing the other circuit at this point, and when the nearer disc turns to close this circuit at 4 the operation is repeated, and so on.

This part of the apparatus aims to accomplish the following results. (1) An easy means of making up a large amount of memory material. If the material is verbal, it is seen that it can be prepared as fast as it can be typewritten. (2) To bring each term of a series suddenly into view and out again. (3) To make the exposures noiselessly. Space will not permit discussing the importance of these points. The second and third have been discussed in connection with the description of other memory apparatus, and it will be understood by all students of the subject. The apparatus is at present not



entirely inaudible to the observer seated at a distance of three feet from this part, when he directs his attention to that noise. It becomes entirely inaudible at once when he directs his attention to memorizing the material that the apparatus exposes. I hope in the future to make some alteration that will make it absolutely inaudible for the distance at which the apparatus is used. The principle employed is that of avoiding the use of any two surfaces striking together in the moving parts. The padded ends of the levers holding the carrier frame hardly move visibly, and the padded surfaces do not break contact in this movement with the sides of the vertical centerpiece.

The part for controlling the exposure intervals is shown in Fig. 3. It consists of a pair of discs with contact teeth, and which are mounted on opposite ends of a common shaft. These discs are turned by a ratchet wheel and a pair of electro-magnets with proper attachments. A metronome with electrical contact attachments may be substituted for this controlling apparatus, if the successive exposure intervals are all to be of the same length. But the present controlling apparatus is intended to give the exposures in pairs, with the two exposures of a pair of unequal length. For example, a term of a series is exposed for one second, when it drops out of sight and the apparatus shows a blank on the card for three seconds, then the next term is shown again for a second followed again by a blank for three seconds, and so on. The nature of the exposure intervals given will depend then on four things: (1) The rate at which the electro-magnets drive the discs; (2) the number of teeth in the ratchet wheel; (3) the number of contact teeth in the discs; (4) the relative setting of the discs on the shaft with reference to how closely a contact on one disc is followed by the next contact on the other disc. It will be seen from a little computation that with only a few pairs of contact discs with different number of teeth and two ratchet wheels a large number of different exposure intervals may be obtained even with only one rate of driving of the discs. The present apparatus was supplied with two ratchet wheels with thirty and thirty-two teeth, respectively, and ten pairs of contact discs

with the following numbers of contact teeth: 1, 2, 3, 4, 5, 6, 8, 10, 15, 16. In Fig. 4 all of the apparatus is shown set up together, and a Zimmermann contact clock is used here to make and break the circuit for the magnets that turn the discs. A metronome might replace the Zimmermann clock, but would not be quite as convenient. With the Zimmermann clock a relay is used to guard the former from injury from the heavier current that the rest of the apparatus requires. This relay is shown in front of the contact clock. This part of the apparatus is not noiseless and should be placed in a sound-proof box or set up in an adjacent room.

The object of following the exposure of each term with a blank exposure of perhaps a different duration is to give an exact and independent control of two processes that are always present in memorizing any kind of material. These processes are the perception, pure and simple, of the term, and its immediate re-imaging or recall, before the next term is given, in the case of successive presentation. I have found in different memory studies that every observer always uses a good portion of the total time, a half to two thirds, for this latter process, when all the material is shown simultaneously. When the terms of the material are shown successively the same holds true, so much so, that an observer will often fail to see a term because his attention is engaged in recalling the one that has just preceded. The possible importance of this immediate recalling process may be seen in the fact that the observer naturally uses so much time for it, and in results which show that the time thus spent may be three times as effective for memory as when that same time is used in further perception merely. This relative importance of the perceptive and the immediately recalling processes, further, seems to vary with the type of the individual observer. It thus becomes quite evident that we should have a means of controlling these two processes independently, so that the experimenter may know just what takes place in the observer's mind. The observer may be instructed as to how to use these two intervals, the exposure of the term and the blank exposure, in order to attain this end.

The apparatus for controlling the retinal image is shown in Fig. 4 on the right. It consists essentially of a large camera with a dark chamber back of the ground glass on which the observer sees the image of whatever the exposing apparatus shows. This dark chamber can be lengthened or shortened independently of any change in the distance between the lens and ground glass. It is supplied with a hood through which the observer looks, not visible in the figure, so that no light strikes his eyes except what comes from the ground glass, thus isolating the observer from all distracting visual stimuli. This arrangement, then, gives a means of varying the size of the image on the retina, and the distance for which the eyes have to be accommodated, independently. The intensity of the light is adjusted with the stop with which the lens is supplied. This part of the apparatus is of course not required if it is not desired to control these factors in the manner described. The parts shown in Figs. 1 and 3 were built by C. H. Stoelting and Co., of Chicago,



78a.

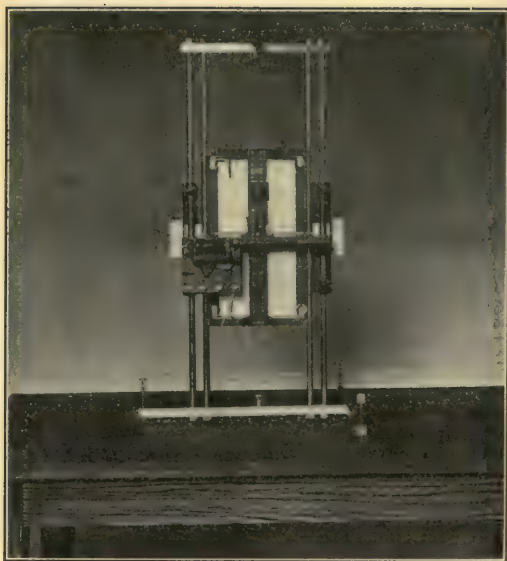


FIG. 1.

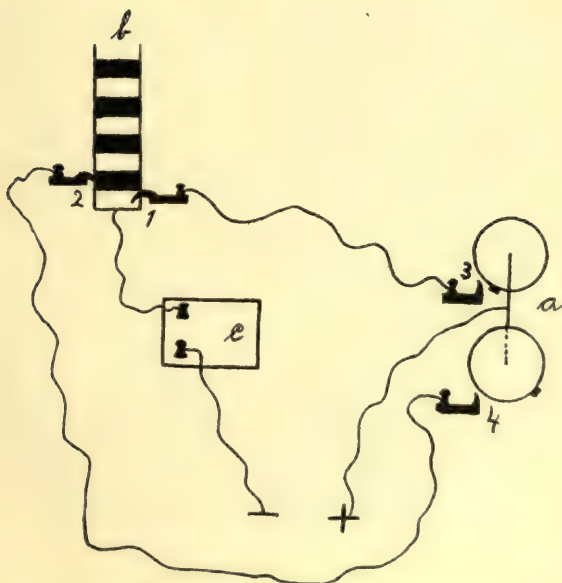


FIG. 2.

PLATE I.



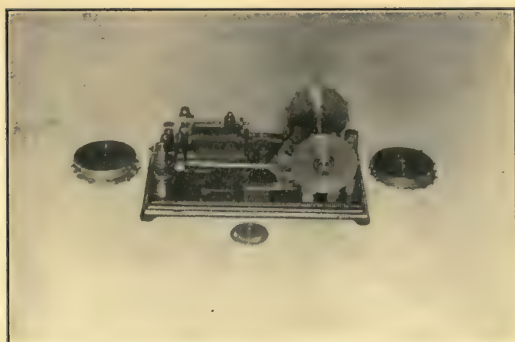


FIG. 3.

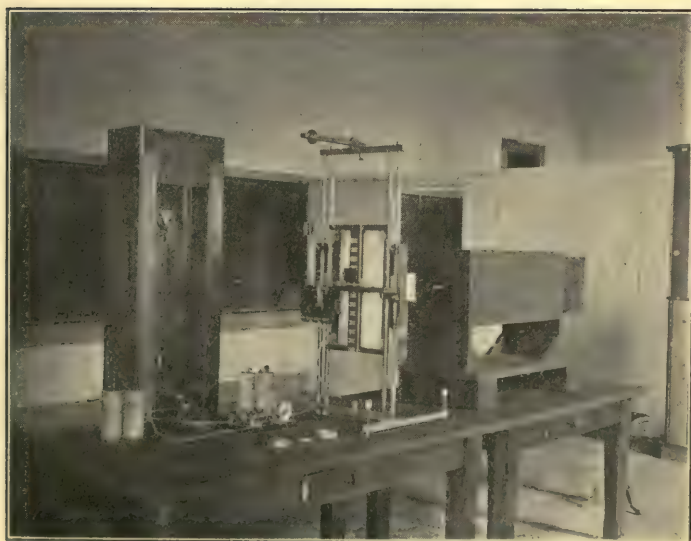


FIG. 4.





# THE PSYCHOLOGICAL REVIEW

---

## THE RELATION BETWEEN MODE OF PRESENTATION AND RETENTION

BY V. A. C. HENMON

*University of Wisconsin*

The relative value of the various methods of presenting material to be learned is a problem of considerable pedagogical and psychological interest. Presentation may be visual auditory, visual-auditory combined, visual-auditory-motor (articulatory or graphic), visual-motor (articulatory or graphic), or auditory-motor (articulatory or graphic). In view of the possible practical application of the results in teaching, the problem has been frequently taken up, particularly the comparison of the values of visual and auditory presentations for immediate memory and the influence of motor factors. Experiments on visual and auditory memory have been reported by Münsterberg and Bigham (18) (method of reconstruction); by Kirkpatrick (12), Hawkins (9), Quantz (21), Smedley (25), Calkins (3), Wissler (29), Netschajeff (19), Kemsies (11), Lobsien (15), W. A. Lay (14), Pohlmann (20), Aliotta (2), Schuyten (23) (method of amount retained or the method of memory span); and by Whitehead (28), Fränkl (7), Segal (24), and von Sybel (29) (committing method). The results of the experiments are not in accord. It is generally claimed that with younger children auditory presentation gives the better results, except for meaningless material, while in older children and adults visual presentation is better than the auditory. This result is attributed to the fact that young children are less familiar with written than with spoken language, that oral methods of teaching

are gradually superseded by methods that make appeal to vision and visual imagery, and that auditory presentation induces greater concentration of attention. The change in relative value of auditory and visual presentation may also be due to an actual change in image type with age. Meumann (16), however, states that visual presentation is far easier for learning both for children and adults and that this rule holds for all material, though more especially for nonsense-syllables.<sup>1</sup> The rule is limited only by type of imagery. Elsewhere, Meumann (16) states that a single method of presentation for all cases can not be declared to be most advantageous.<sup>2</sup> The value of a method of presentation varies with the nature of the material, the type of imagery of the learner and the procedure in presentation.

Less attention has been given to the effect of simultaneous presentation to several senses and to the relation of methods of presentation to secondary memory. On general psychological grounds it might be expected that combining simultaneously various methods of presentation would be an advantage, particularly with stimuli, such as words, which could, if necessary, be learned naturally through visual or auditory presentation. There would supposedly result an increase in the number of associations, which would facilitate retention and recall. If the visual cue is lacking, the auditory or the motor cue might be effective. On the other hand, too great an increase in the number of possible cues may be distracting and reduce the force of associations. This might be expected where the natural appeal is to one sense, as in colors or tones.

The results of experimental investigation are inconclusive. Münsterberg and Bigham (18), found that "a series of presentations offered to two senses at the same time is much more easily reproduced than if given only to sight or only to hearing." The percentages of error in reconstruction of series of numbers and colors were as follows:

<sup>1</sup> P. 25.

<sup>2</sup> P. 121.



	Per Cent.
10 numbers heard.....	14.1
10 numbers seen.....	10.5
10 numbers seen and heard at the same time.....	3.9
10 colors named.....	29.3
10 colors seen.....	17.9
10 colors seen and named at the same time.....	4.9

There is thus a significant superiority in the combined method. When taken alone "visual memory excels strongly the aural."

Quantz (21), however, determined the visual and auditory memory-spans for words and notes that "the use of eye and ear together, the words being read aloud by the subject, is little advantage over either separately, when the words are read to him or silently by him." In fact, the combined presentation may be a hindrance.

W. A. Lay (14), in several researches on methods in teaching spelling, has studied the influence of kinesthetic factors (articulatory and graphic) combined with visual and auditory presentation in apprehension and retention of nonsense syllables and numbers. In all cases the suppression of articulatory movements increased the errors, while the use of graphic or articulatory movements reduced the errors, an effect which might be due to the indirect influence on attention rather than the direct influence of the kinesthetic factors themselves.

Fuchs and Haggmüller (8) and Itschner (10) repeated Lay's experiments under more constant conditions and did not find that articulatory or graphic movements played the important part in learning which Lay attributed to them. Abbott (1) in a study of memory consciousness in orthography finds that "irrespective of the method of presentation and the manner of learning, the typical mode of recall for all observers was through the visual imagery of the letters." Vocalization is always used and aids in learning the spelling of a word by determining the correct pronunciation of it and thus arousing auditory imagery and by facilitating accurate visual perception.

Cohn (4) tested the coöperation of visual, auditory and

motor factors, with especial reference to motor factors, in the memorizing of lists of twelve consonants. In one set of experiments subjects read the consonants aloud, in the second speech movements were suppressed, and in the third numbers or vowels were pronounced during the reading of the consonants. All the subjects remembered most by the first method and least by the last. Cohn's results are thus similar to those of Münsterberg and Bigham.

Smedley (25) in tests of immediate memory for digits on Chicago school children found auditory-visual memory better than either alone, that the visual-auditory-motor (articulatory) was better than the auditory-visual, and that the visual-auditory-motor (graphic) was inferior to both the auditory-visual and the visual-auditory-motor (articulatory).

Kensies (11) tested auditory, visual and visual-auditory presentations of Latin words and nonsense words with school children and found the auditory presentation to be best in all cases. The combined method usually gave poorer results than with visual or auditory presentation alone.

Finzi (6) noted that in visual presentation of letters, numbers and nonsense-syllables the subjects might employ for retention visual images alone, or the auditory and articulatory images combined, or the articulatory images alone. He found that retention by means of visual images gave the most reliable results.

Pohlmann (20), in the most complete investigation of sensory modes of presentation thus far by the method of amount retained, studied the effect of visual, auditory, visual-auditory and visual-auditory-motor presentations of objects, words, nonsense-syllables and numbers on school children from 9 to 14 years of age. He found that auditory presentation is better than visual with significant material (words) but that the visual presentation is better with nonsense material (numbers and syllables). The value of visual presentation for words increases with age and finally surpasses the auditory. The combined visual-auditory presentation shows on the average in all cases a slightly better result than with the auditory or the visual alone. The visual-auditory-

motor presentation gives poorer results with three repetitions and with one presentation very much poorer results. His percentages of amount retained, omitting the results on objects, are as follows:

	Auditory	Visual	Visual-Auditory	Visual-Auditory-Motor
Words.....	55 $\frac{1}{6}$	50 $\frac{4}{6}$	56 $\frac{1}{2}$	49 $\frac{1}{2}$
Nonsense syllables..	42	53 $\frac{32}{60}$	53 $\frac{51}{60}$	52 $\frac{5}{16}$
Numbers.....	47 $\frac{1}{4}$	54	54	51 $\frac{5}{12}$

Fränkl (7) using the committing method with eight syllable series tested the values of auditory, auditory-motor, visual-auditory-motor and visual presentations. He found that the visual presentation was better with visual types, auditory with auditory types.

Schuyten (23) in class experiments with eight two-place numbers found auditory presentation better than visual-auditory.

Segal (24) found that visual presentation was best for those of visual type, auditory presentation for the auditory types. Meumann (17) concludes from his own observations and those of Segal that, in learning, reliance upon the natural type of imagery is better than the use of a combined method. He points out that it is not the number of associations but the strength of associations that determines retention and recall.<sup>1</sup>

Von Sybel (27) in a recent study compared visual, visual-motor, auditory, auditory-motor, visual-auditory and visual-auditory-motor presentations using a combination of the committing method and the method of right associates. The experiments were made on seventeen subjects with nonsense-syllables. He finds that reading aloud (visual-auditory-motor) is better for learning in almost all cases than silent reading (visual). This result holds even with those of visual image type except with slow rates of presentation where silent reading gives best results. The number of right associates, however, is practically always greater with silent reading, regardless of image type.

<sup>1</sup> P. 88.



Visual-auditory presentation is almost without exception better for learning than the visual but retention is better with visual presentation. All subjects considered visual-auditory presentation to be distracting and expected poorer results, an expectation which was not borne out. Visual presentation with articulation gave slightly better results for learning than without articulation but right associates were more numerous when articulatory movements were suppressed. Auditory presentation with articulation gave practically the same result as without articulation. Auditory presentation gave without exception better results than the visual. This was true both for auditory types and also for the dominantly visual.

This summary of the available evidence bears out the statement that the results on the effect of methods of presentation on learning and retention are not in accord.

The early experimenters apparently assumed that tests with various methods of presentation measured the efficiency of the visual, auditory and motor memories. However, it is clear that the method of presentation is not necessarily an index of the imagery employed. The method of learning depends in part on the method of presentation, in part on the sort of material and in part on the imagery of the learner. A list of words may be read to three subjects and if they represent three distinct types of imagery the audile will recall the list by auditory images, the visual will translate the words into visual images and the motor will speak the words internally. Since the great majority of individuals are of the mixed type of imagery, using now one form of imagery and now another, the memory-images employed will vary with the sort of stimuli used, the mode of presentation and the degree of dominance of one form of imagery in this mixed type. Visual stimuli will, other things being equal, be better remembered visually, auditory stimuli by auditory images. The mode of presentation may thus determine the method of learning. Similarly the nature of the material may determine the method of learning.

In view of these considerations it is necessary to distinguish

between the psychological and the pedagogical problems involved. Pedagogically the problem is largely quantitative. How much can be retained by each method of presentation and how accurate is the retention? The interest is in the results of the presentation rather than in the processes by which the presented material is retained. Psychologically the problem is largely qualitative. How is the material apprehended, retained and reproduced and how does the nature of apprehension, retention and reproduction vary with the type of imagery, the mode and rate of presentation and the sort of material used. In experiments, as Meumann (17) points out, we may approximate as closely as possible actual school conditions but in such cases we are not in a satisfactory position to study the processes employed in learning. Or we may instruct subjects to use auditory imagery with auditory presentation, visual imagery with visual presentation, etc., but then we may be interfering with the natural method of learning. In the first case we discover how the various modes of presentation actually affect learning but the analysis and explanation of the results is difficult. In the second case we get definite results of interest to psychology but the practical application to school conditions is doubtful.

#### EXPERIMENTS AND RESULTS

The following experiments were designed to test the influence of visual, auditory, visual-auditory and visual-auditory-motor (articulatory) presentations on retention. Since the value of a method of presentation may vary with the kind of material used, with the number of repetitions, and with different individuals, the experiments were made with three sorts of material, concrete nouns, two-place numbers and nonsense-syllables, with one, two and three repetitions and on six subjects. Precautions were taken to secure, as far as possible, uniformity in the material. The nouns were all of four letters each, arranged so as to avoid placing words in succession that were similar in sound or appearance. No word began with the same letter which was found either at the beginning or end of the word preceding it.

In the lists of numbers no consecutive numbers began or ended with the same figure. The same figure was not used twice in any one number; the zero was not used at all; and all multiples and divisors of a number in any list were avoided. The usual precautions were taken with the lists of nonsense-syllables. To avoid difficulties in the case of auditory presentation syllables beginning or ending with c, q, and h, and syllables beginning with x, were rejected. The syllables were all of three letters.

The method employed was that of amount retained (*Methode der behaltenen Glieder*). Each series consisted of ten members, typewritten on strips of paper which could be fastened around the drum of a kymograph. The rotation of the drum behind the screen before which the subjects were seated was kept at a uniform rate and permitted the exposure of each member of a series for three quarters of a second with an interval of one and one half seconds between successive members. In the visual presentation the subjects read the stimuli directly from the rotating drum and immediately wrote down as many members as could be recalled and in the order presented. The subjects were asked to repress movements of articulation. In the auditory presentation the experimenter read the stimuli from the drum, the subject keeping his eyes closed and repressing movements of articulation. In the visual-auditory presentation the subject both saw the stimuli and heard them read by the experimenter. In the visual-auditory-motor presentation the subject himself read the lists aloud. A double fatigue order was observed in number of presentations and in order of materials and in modes of presentation.

Six subjects took part in the experiment. All had had a year or more of laboratory training in psychology. I am particularly indebted to Mr. Carl L. Rahn, sometime instructor in psychology at the University of Colorado, for his assistance both as subject and as experimenter, and to Miss Mary E. Lakenan, assistant in psychology, at the same institution, for aid both as subject and in the calculation of the results. The experiments were made during the year



TABLE I

## ONE PRESENTATION

Subjects	Nouns										Syllables						Numbers													
	V.					V.A.					V.A.M.					V.					V.A.					V.A.M.				
	m.v.	A.	m.v.	V.A.	m.v.	m.v.	A.	m.v.	V.A.	m.v.	V.A.M.	m.v.	V.	m.v.	A.	m.v.	V.A.	m.v.	V.	m.v.	A.	m.v.	V.A.	m.v.	V.A.M.	m.v.				
A	42.7	9	59.5	10	53.3	8	56.4	6	17.4	9	36.0	8	32.1	11	30.5	9	19.9	7	27.4	3	31.3	5	33.2	8	33.2	5				
B	48.0	8	60.0	11	59.7	14	57.3	6	38.2	6	37.4	9	34.6	7	34.1	4	56.0	17	54.2	11	53.8	12	58.0	15	58.0	15				
C	45.3	8	57.2	5	50.5	10	61.6	7	25.5	5	35.1	9	41.3	9	38.9	6	40.7	7	45.7	11	44.3	10	44.0	9	44.0	9				
D	50.7	9	60.9	9	68.1	13	68.7	12	28.8	6	37.1	6	35.9	9	40.1	5	25.6	7	33.6	7	36.6	5	33.3	9	33.3	9				
E	23.8	5	35.1	4	36.7	5	30.4	5	13.6	6	18.1	5	19.4	5	15.8	4	28.2	7	38.9	10	32.6	5	32.8	5	32.8	5				
F	41.7	10	47.8	4	56.5	10	50.1	9	26.9	8	35.5	4	35.7	8	36.2	8	47.8	11	56.1	14	49.5	8	44.9	9	44.9	9				
Av.	42.0	8.1	53.4	7.2	54.1	10.0	54.0	7.5	23.4	6.7	33.2	6.8	33.1	8.2	32.6	6.0	36.3	9.3	42.6	9.3	41.3	7.5	41.0	9.1	41.0	9.1				

## TWO PRESENTATIONS

A	56.8	17	72.3	13	70.0	8	67.4	14	31.3	7	50.1	12	43.6	10	34.7	8	22.6	9	35.7	7	28.4	6	33.7	7	33.7	6	33.7	7	33.7	6	33.7	7
B	62.8	13	71.1	14	70.0	11	74.8	14	39.7	11	40.7	9	45.0	15	41.1	14	57.5	15	66.0	11	69.4	15	60.1	12	60.1	15	60.1	12	60.1	15	60.1	12
C	45.6	11	64.7	3	62.0	9	60.6	8	32.0	8	38.6	9	35.3	10	39.3	7	45.7	12	54.2	12	47.4	12	52.1	11	52.1	12	52.1	11	52.1	12	52.1	11
D	77.9	17	73.9	15	90.9	9	74.1	13	30.9	11	31.6	6	35.6	8	45.6	10	32.4	6	41.3	11	44.9	8	38.7	7	38.7	8	38.7	7	38.7	8	38.7	7
E	38.4	8	43.7	6	48.9	7	39.3	9	19.0	7	22.5	10	17.9	4	17.1	5	34.1	7	35.0	7	39.8	8	36.8	7	36.8	8	36.8	7	36.8	8	36.8	7
F	61.1	12	59.5	17	75.5	12	68.9	10	37.2	4	36.0	6	41.5	5	40.6	7	48.9	7	55.8	14	55.5	11	54.6	20	54.6	11	54.6	20	54.6	11	54.6	20
Av.	57.1	13.0	64.2	11.3	71.2	9.3	64.1	11.3	31.7	8.0	36.5	8.7	36.4	8.7	36.4	8.5	40.2	9.3	48.0	10.3	47.5	10.0	46.0	10.6	46.0	10.0	46.0	10.6	46.0	10.0	46.0	10.6

## THREE PRESENTATIONS

A	64.1	12	70.0	10	84.4	8	79.8	11	32.2	13	44.8	11	41.2	14	45.4	10	26.2	10	36.1	9	33.5	9	34.9	9	34.9	9	34.9	9	34.9	9	34.9	9
B	81.5	11	78.2	11	80.8	13	80.9	13	43.8	11	44.8	8	45.6	12	54.3	18	67.3	15	78.2	12	75.1	16	76.0	14	76.0	16	76.0	14	76.0	16	76.0	14
C	50.3	10	68.7	10	63.4	7	64.0	11	28.9	7	45.7	10	41.2	6	44.7	5	50.6	11	60.9	8	54.6	9	52.9	9	52.9	9	52.9	9	52.9	9	52.9	9
D	84.3	10	71.9	10	85.9	14	80.2	17	43.3	18	39.5	14	42.4	19	59.5	13	49.8	11	46.5	13	46.7	13	44.9	7	44.9	13	44.9	7	44.9	13	44.9	7
E	40.6	7	51.1	12	51.5	5	41.9	7	16.0	4	29.0	6	25.0	7	23.2	6	38.4	11	58.8	14	37.8	6	43.7	8	43.7	6	43.7	8	43.7	6	43.7	8
F	63.9	15	71.4	12	66.6	10	71.4	14	31.5	12	41.5	9	45.3	8	42.7	12	55.8	8	68.0	12	58.4	10	55.4	9	55.4	10	55.4	9	55.4	10	55.4	9
Av.	64.1	10.8	68.6	10.8	72.1	9.5	69.7	12.2	32.4	10.7	40.8	9.7	40.1	11.0	43.4	10.7	48.0	11.0	58.0	11.3	50.5	8.8	51.3	9.3	51.3	8.8	51.3	9.3	51.3	8.8	51.3	9.3

1908-1909. All of the subjects were of the mixed type of imagery, *A*, being markedly auditory-motor, and the remainder very markedly visual.

The gross results of the experiments appear in Table I. It gives in summary the average percentages of the series retained for one, two and three presentations, for the three sorts of material used, and the six subjects, *A*, *B*, *C*, *D*, *E*, and *F*, the mean variations and the general averages for the six subjects. Each figure in the table, except the general averages, represents an average from 10 experiments. The figures, therefore, possess a high degree of reliability, the maximum probable error being .056 and the minimum .008, with an average probable error of .027.

#### I. COMPARISON OF VISUAL AND AUDITORY PRESENTATIONS

The most striking result of the experiments is the marked superiority of the auditory over the visual presentation. This result holds in all but six of the fifty-four cases shown in the table, *B* giving a slightly better average for visual presentation of syllables and numbers with one presentation, and *D* giving a better average for visual presentation of all materials with three presentations. *B* and *D* are markedly visual in image type. All of the remaining subjects, except *A*, are also visual. The result is, therefore, surprising and not easily accounted for.

The fact that subjects of the visual type retain more with auditory presentation indicates either that image type is not a significant factor in determining the value of the mode of presentation, or that visualization and retention are more accurate when stimuli are heard than when read. The latter alternative seems the more probable. The introspections show that many subjects of the visual type tend always to visualize the stimuli and that greater freedom in visualization is possible with auditory presentation. Visual presentation is distracting and puts a constraint on visual imagery. This is especially true when stimuli are presented on a rotating drum, which increases the difficulty for the

visualizer of arranging a series quickly and without confusion. If the stimuli were exposed successively in different locations in the field it seems likely that the visual presentation would show to better advantage. This appears to me to account in part for the superiority of auditory presentation in my results and in those of von Sybel. The method of presentation ordinarily employed in memory experiments, for visual presentation is more artificial than for the auditory, and hence tends to favor auditory presentation. Moreover, the influence of the constant practice acquired by adult students in retention from auditory presentation is an important factor.

## 2. COMPARISON OF VISUAL AND AUDITORY PRESENTATION WITH DIFFERENT MATERIALS

The superiority of the auditory presentation over the visual holds for all the materials used, being quite as great for nonsense-syllables and numbers as for nouns. This result does not accord with that of Pohlmann, who found, as indicated above, that the visual is much superior to the auditory presentation with nonsense-syllables and numbers but inferior with nouns. He found, moreover, that even with nouns the relative value of auditory presentation decreases with age and that ultimately visual presentation surpasses the auditory with nouns also.

Pohlmann used three presentations only with nonsense-syllables. The stimuli were presented at intervals of two seconds. His experiments were class tests on school children who had no familiarity with nonsense-syllables. Moreover, the syllables included diphthongs and digraphs and hence were more difficult of utterance and more difficult to apprehend from oral presentation. This difference in conditions no doubt accounts for the difference in results.

Class tests, such as those of Pohlmann, seem to me to have little value. Pohlmann points out the obvious reasons why the experiments throw little light on the psychological processes involved, but suggests that they may have a pedagogical value. But the pedagogical value of methods of presentation must be determined not by a single test on groups



of individuals but by repeated tests on the same individual. From a pedagogical point of view we are interested in the best and most economical mode of presentation shown after practice and in the long run. A single test with nonsense-syllables on school children, to whom such stimuli are unfamiliar, may show visual presentation to be superior, with ten tests or more the auditory may be superior and pedagogically this would be the fact worth knowing. It seems likely from some brief experiments which I have made with children of the same age as in Pohlmann's experiments that the superiority of auditory presentation would be shown with them also.

Kuhlmann's (13) review of the evidence leads him to the conclusion that "visual presentation of meaningless material is always better than auditory presentation." However, the careful study of Kemsies (11) showed the superiority of auditory presentation quantitatively and qualitatively both for Latin words and nonsense-words. Hawkins (9) found auditory presentation of words superior with children. Von Sybel (27), likewise, found that the number of repetitions required for learning was less for auditory presentation. My own results point clearly to the superiority of auditory presentation.

### 3. VISUAL-AUDITORY PRESENTATION

The visual-auditory mode of presentation, where the stimuli are presented simultaneously to the eye and the ear, is superior to the visual presentation alone in all but seven out of the fifty-four cases, or 87 per cent. In the general averages the superiority holds in all cases. The combined method is superior to the auditory alone in but twenty-five of the fifty-four cases, or 46 per cent. In the general averages the combined method gives better results with nouns but with syllables and numbers there is practically no difference, the combined method being slightly inferior. This result is in substantial agreement with that of Pohlmann and other recent investigators. Von Sybel found that the number of repetitions required for learning was less with visual-auditory

presentation than with visual presentation but that the number of right associates (Treffer) was greater with visual presentation. His data do not enable him to compare visual-auditory with auditory presentation. His subjects noted the distraction arising from the twofold division of attention, from the fact that the visual and the auditory presentations were not simultaneous, and from the differences in pronunciation of the syllables. In spite of these distractions, which led most of the subjects to feel that visual-auditory presentation was less effective than the visual alone, the results showed an advantage in favor of the visual-auditory method.

#### 4. VISUAL-AUDITORY-MOTOR PRESENTATION

Visual-auditory-motor presentation, in which the subject himself reads the stimuli aloud, is inferior to the visual-auditory in thirty-two out of fifty-four cases, or 59 per cent.; to the auditory alone in thirty cases, or 55 per cent.; and to the visual alone in eight cases, or 15 per cent. In the general averages the differences between the visual-auditory-motor, the visual-auditory, and the auditory presentations are very slight, while all are superior to the visual alone. Simultaneous appeal therefore to the several senses is no advantage for retention. This result is in agreement with that of Pohlmann. Pohlmann, however, apparently attributes the inferior results to the distraction caused by the simultaneous speaking of the pupils in the class and not to the lack of reinforcement by motor factors. That this is not the true reason is shown by the fact that in my experiments the subject alone read the stimuli aloud, and this possible factor is eliminated.

The studies made by Cohn, Smedley, Lay, Smith, Aliotta and von Sybel have all shown the superiority of the visual-auditory-motor (articulation) presentation over other methods, and it is generally claimed that vocalization of what is to be learned is an aid to memory. Pohlmann, Colvin, Fuchs and Haggmüller find the value of articulation to be slight. Colvin (5) concludes that "the importance of motor

imagery, both for the hand and for the vocal organs, appears to be much less than has generally been supposed. . . . Except in pronounced cases where the child is extremely motor in his way of thinking, children seem to depend but little on their motor imagery; indeed, the kinesthetic sensations from the throat and hand may be a hindrance rather than an aid in learning." This conclusion the writer would extend to adults so far as immediate memory in relation to articulatory sensations is concerned. Vocalization may be an aid in inducing attention when there is a tendency for it to wander and may bring about clearness of perception of details with unfamiliar material. Where this is unnecessary vocalization is distracting and of no assistance in memorizing.

### 5. INFLUENCE OF REPETITIONS

The relative value of the different modes of presentation remains unchanged for one, two and three presentations.

The auditory presentation is superior to the visual in all cases. The difference in amount retained for nouns are 11.4 per cent., 7.1 per cent., and 4.4 per cent., respectively; for syllables, 9.8 per cent., 4.9 per cent., and 8.4 per cent.; and for numbers, 6.3 per cent., 7.8 per cent., and 10 per cent. It seems, therefore, that the value of the auditory presentation is greater for one presentation with nouns and decreases with an increase in repetitions while with numbers the reverse is the case.

With all of the other forms of presentation there is practically a uniform increase in amount retained with an increase in the number of presentations, and the relative values are constant for all materials. There is no evidence in support of Hawkins' conclusion that the second presentation gives a poorer result than one. In seven cases out of seventy-two a slightly better result was obtained with one presentation than with two.

### 6. INDIVIDUAL DIFFERENCES

The individual differences in amount retained are considerable even in such a highly selected group, the range



being approximately as 2 : 1. The differences are practically constant for the various modes of presentation contrary to expectations. If one averages the percentages retained for visual, auditory, visual-auditory and visual-auditory-motor presentations separately, combining the results with the different materials and with the three presentations, which would serve as a rough measure of the individual's performance, or better still, if one averages the ranks attained under these conditions, the stations of the six individuals are practically identical for the four methods of presentation. In other words, the correlations in abilities with the different forms of presentation are practically perfect. Superiority with one form of presentation means practically the same degree of superiority with others. This result is contrary to a common belief that superiority with one form of presentation is correlated with inferiority or a much lower degree of superiority in others. The closeness of the correlation is no doubt due largely to the fact that with practiced adults the natural method of learning is the same no matter what the form of presentation may be. Image type is a factor of some influence as is shown by the record of subject *A*, who is auditory-motor in type and whose station is relatively better with the auditory than with the other presentations.

The correlation between abilities with different materials are not as close as might be expected, for, after all, the differences for learning between nouns, syllables and numbers are not great. The main facts can easily be seen from a table of the ranks, for the different individuals, based on the average of all percentages for nouns, syllables and numbers separately, or on an average of the ranks attained in the different experiments.

	Nouns	Syllables	Numbers
<i>A</i>	3	5	6
<i>B</i>	2	1	1
<i>C</i>	5	4	3
<i>D</i>	1	2	4
<i>E</i>	6	6	5
<i>F</i>	4	3	2

The correlations between syllables and numbers and between nouns and syllables is high, the coefficient of correlation by the method of rank-differences being in each case  $+.77$ . The coefficient for nouns and numbers is  $+.20$ . The number of cases is too small to attach much significance to the figures but they represent roughly the amount of correlation. Subject *D* is clearly of the so-called ingenious type and her record with nouns is much superior to that with nonsense-syllables and numbers, the absence of associations with numbers causing especial difficulty.

### 7. CONCLUSIONS

The following summary sets forth the main conclusions of this study:

1. Auditory presentation is clearly superior to visual presentation in immediate memory of adults, a result attributable to the greater ease and freedom of visualization with auditory presentation and the greater effort of attention required.

2. This superiority of auditory over visual presentation holds for all materials (nouns, nonsense-syllables, numbers), for all subjects irrespective of image type, and for one, two and three presentations. This result is not in accord with the opinion commonly held that visual presentation is superior, especially with meaningless material.

3. Combined visual-auditory presentation is slightly inferior to the auditory alone and decidedly superior to the visual alone. The advantage of a combined method is very much less than that shown in earlier investigations.

4. Visual-auditory-motor presentation is slightly inferior to the auditory and the visual-auditory presentations and superior to the visual alone. Articulation or vocalization is of little value for immediate memory.

5. The correlations of abilities with different forms of presentation are positive and very high, superiority with one indicating practically the same degree of superiority with another.

## BIBLIOGRAPHY

1. ABBOTT, E. E. Memory Consciousness in Orthography. *PSYCH. REV. Mon. Suppl.*, Vol. II., 1909.
2. ALIOTTA, ANT. Esperimenti sulla memoria immediata. *Rivista di Psicologia*, I., 1905.
3. CALKINS, M. W. A Study of Immediate and Delayed Recall of the Concrete and of the Verbal. *PSYCHOL. REV.*, V., 1898, pp. 457-956.
4. COHN, JONAS. Experimentelle Untersuchungen über das Zusammenwirken des akustisch-motorischen und des visuellen Gedächtnisses. *Zeitschrift für Psychol. und Physiol. der Sinnesorgane*, XV., 1897, pp. 161-183.
5. COLVIN, S. S., AND MYERS, E. J. Development of Imagination. *PSYCHOL. REV. Mon. Suppl.*, No. 44, 1909.
6. FINZI, J. Zur Untersuchung der Auffassungs- und Merkfähigkeit, *Kraepelin Psychol. Arbeiten*, III, 1900.
7. FRÄNKEL, E. *Über Vorstellungselemente und Aufmerksamkeit*. Augsburg, 1905.
8. FUCHS, H., AND HAGGENMÜLLER, A. Studien und Versuche über die Erlernung der Orthographie. *Sammlung von Abhandlungen a. d. Gebiete der pädagog. Psychol.*, II., 1898.
9. HAWKINS, C. J. Experiments on Memory Types. *PSYCHOL. REV.*, IV., 1897, pp. 289-294.
10. ITSCHNER, H. Lay's Rechtschreib-Reform. *Jahrbuch des Vereins f. wissenschaft. Pädagogik*, XXXII., 1900.
11. KEMSIES, F. Gedächtnisuntersuchungen an Schulkindern. *Zeitschrift für pädagog. Psychol.*, II., III., 1900-1901.
12. KIRKPATRICK, E. A. Experimental Study on Memory. *PSYCHOL. REV.*, I., 1894, pp. 602-609.
13. KUHLMANN, F. The Present Status of Memory Investigation. *PSYCHOL. BULLETIN*, V., 1908.
14. LAY, W. A. *Experimentelle Didaktik*, 3d ed., 1910, pp. 297-305; 351-370.
15. LOBSIEN, M. Experimentelle Untersuchungen über die Gedächtnisentwicklung bei Schulkindern. *Zeitschrift für Psychol. und Physiol. der Sinnesorgane*, Bd. 27, 1901, pp. 34-76.
16. MEUMANN, E. *Vorlesungen zur Einführung in die Experimentelle Pädagogik*, 1907.
17. MEUMANN, E. *Technik und Ökonomie des Gedächtnisses*, 1908.
18. MÜNSTERBERG, H., AND BIGHAM, J. Memory. *PSYCHOL. REV.*, I., 1904, pp. 34-38.
19. NETSCHAJEFF, A. Experimentelle Untersuchungen über die Gedächtnisentwicklung bei Schulkindern. *Zeitschrift für Psychol. und Physiol. der Sinnesorgane*, Bd. 24, 1900, pp. 321-351.
20. POHLMANN, A. *Experimentelle Beiträge zur Lehre vom Gedächtnis*, 1905.
21. QUANTZ, J. O. Problems in the Psychology of Reading. *PSYCHOL. REV. Mon. Suppl.*, No. 5, Dec., 1897.
22. REUTHER, F. Beiträge zur Gedächtnisforschung. *Psych. Studien*, I., 1906, pp. 4-101.
23. SCHUYTEN, M. C. Sur la validité de l'enseignement intuitif primaire. *Archives des Psychol.*, V., 1906.
24. SEGAL, J. Über den Reproduktionstypus und das Reproduzieren von Vorstellungen. *Archiv f. d. ges. Psychol.*, XII., 1-3, 1908.



25. SMEDLEY, C. Report of Department of Child-Study and Pedagogic Investigation, Chicago, No. 3, 1900-01.
26. SMITH, T. L. On Muscular Memory. *Am. Jour. Psychol.*, Vol. VII., 1896.
27. VON SYBEL, A. Über das Zusammenwirken verschiedener Sinnesgebiete bei Gedächtnisleistungen. *Zeitschrift für Psychol.*, Vol. 53, 1909.
28. WHITEHEAD, L. G. A Study of Visual and Aural Memory Process. *PSYCHOL. REV.*, III., 1896.
29. WISSLER, C. Correlation of Mental and Physical Tests. *PSYCHOL. REV. Mon. Suppl.*, III., 1906.

# COMBINING THE RESULTS OF SEVERAL TESTS: A STUDY IN STATISTICAL METHOD

BY R. S. WOODWORTH

*Columbia University*

When several tests have been made of the same individuals, it may be desirable to combine the results so as not only to get group averages and coefficients of correlation, but also to show the success of *each individual* in the series of tests taken as a whole. A prevalent custom, in such cases, is to drop from quantitative to qualitative statements, and to say, for example, that an individual who has done very well in the first test, well in the second, but rather poorly in the third, has on the whole, therefore, done rather well. A somewhat more quantitative statement can be got by ascertaining in what proportion of the tests an individual stands above the average, and a still better plan is to transmute the original measures into an 'order of merit' for each test, so as to be able to state, for example, that an individual stood first in one test, fifth in another and fifteenth in a third, and had accordingly an average rank of seventh. This method (in the hands of Cattell and others) has proved of much value where absolute measures are impracticable; but to transmute absolute measures, already obtained, into an order of merit is to throw away part of the information contained in the measures. What is needed is a method of combining results which shall preserve all the refinement of the original measurements. Such a method exists, and is certainly familiar to statisticians; but it seems to be overlooked in many cases where it would prove of value.

What is here attempted is (1) to win favor for the method (credit for its invention being expressly disclaimed); and (2) to work out simplified formulæ which can be used for computing correlations, when the method has been employed. The method itself gives each individual's average standing in any

number of tests; once this has been found, the additional labor of computing correlations is slight.

## I

The problem, once more, is as follows: results being at hand from several tests of the same individuals, it is desired to combine them so as to measure the success of each individual in the tests as a whole.<sup>1</sup> This may be difficult, either because the measurements afforded by the different tests are incommensurable (one being, perhaps, in terms of time and another in terms of accuracy), or because the average for one test is very different from that for another test.

Suppose, for example, that the group average for one test is 10 seconds, and for another test 100 seconds; and suppose that an individual's records in the two are respectively 8 and 70 seconds. How shall we combine his records? If we simply take the average of 8 and 70 seconds, we obtain a hybrid value with so great a probable error as to make it useless for further deductions. If we express each of his records in

<sup>1</sup> Essentially the same problem may arise in other forms. The general problem may be stated as follows: We have several series of measurements, and some principle of correspondence which enables us to connect each measurement in one series with one and only one measurement in each of the other series; and it is desired to combine the corresponding measurements. The writer has met this problem in several different forms. On one occasion I desired to find the relative difficulty of a number of words as stimuli for the 'opposites test.' I tried the words on several individuals, obtaining the association time for each word. Now as some of my subjects were much slower than others, and varied much more from word to word, simply averaging the association times for each word would not do equal justice to all the individuals, but the resulting differences between the words would be determined mostly by the slow, variable individuals. I desired some method of giving each individual, whatever his speed and variability, the same share in determining the average result; and I was finally led to the method described in the text. Again, in studying the work curve for very short periods of activity, I made use of lists of twenty words, the subject being required to respond to each word by a word standing in some assigned logical relation to it, and the time being taken for each single reaction. The subject went through twenty such lists, all different, and I desired to find whether the position of a reaction in the series of twenty affected its speed. But the lists of stimuli were, unavoidably, of unequal difficulty, and the variation within one list was greater than in another. If, then, I simply found the average reaction time for the first words, etc., and compared these averages, my comparison would not be based equally on all the experiments, but mostly on those that showed the greatest internal variation. What I desired was a means of giving each experiment an equal influence on the total result; and this was accomplished by the method given in the text.



terms of the corresponding group averages, as 80 and 70 per cent. respectively, we seem, indeed, to avoid most of the spurious unreliability, and we seem also to find that the individual did better in the first test than in the second. But this procedure is not justified, except on the assumption that the variability of a group in different tests is proportional to the group average; for, otherwise, a good share of the group may, in one test, take less than 70 per cent. of the average time, whereas in another test only an exceptionally good record may be as low as this, so that such a mark as 70 per cent. of the average may mean very different degrees of proficiency in different tests.

Now, as a matter of fact, there is no general law that the group variability is proportional to the group average. Thorndike<sup>1</sup> has shown that no uniform relation holds between the average and the variability, and has brought forward, in particular, cases in which the variability increases more slowly than the average. When the length of a test, consisting of a series of similar reactions, is increased, the variability does not increase as fast as the average, because chance variations in the elementary reactions tend more and more to compensate for one another as their number is increased. But suppose—and this is a point not hitherto made—that not the length of the series of reactions, but the difficulty of the single reactions is increased—then one would expect the variability to increase faster than the average.

One would expect adults to differ proportionately more in long division than in easy addition, for all would have kept up some practice in the latter, while in the former some would still be in practice and some entirely out of practice. If, indeed, the difficulty of the test were still further increased—imagine, for example, a test in extracting the cube root—both the variability of the group and the average (as measured by time of performance) would be indefinitely increased because of the presence of some individuals who would fail utterly; and in this case the proportionate variability could not be calculated. But within a moderate range of difficulty

<sup>1</sup> 'Empirical Studies in the Theory of Measurements,' 1907, p. 9.

of the test, the variability should increase more rapidly than the average time of performance. The following table of results from a series of tests<sup>1</sup> tends to favor this conclusion, since, within each class of tests, the easiest have the lowest proportionate variability. Certainly the proportionate variability differs greatly from one test to another, and accordingly any statistical treatment which assumes equal relative variability is inaccurate.

	Group Average Time per Single Reaction	$\sigma$	$\frac{\sigma}{\text{Av.}}$
1. Logical relations:			
Opposites . . . . .	1.23	.17	.14
Agent-action . . . . .	1.30	.23	.18
Verb-object . . . . .	1.39	.21	.15
Part-whole . . . . .	1.53	.37	.24
Attribute-substance . . . . .	1.53	.47	.31
Supraordinate concept . . . . .	1.54	.37	.24
Action-agent . . . . .	1.55	.41	.27
Whole-part . . . . .	1.57	.36	.23
Subordinate concept . . . . .	1.84	.42	.23
2. Mixed relations . . . . .	3.14	.53	.17
3. Addition, Kraepelin test . . . . .	1.14	.26	.23
4. Constant increment: Add 4 . . . . .	1.36	.30	.22
Subtr. 4 . . . . .	1.64	.54	.33
Add 17 . . . . .	3.90	1.18	.30
5. Color naming . . . . .	.59	.10	.17
Form naming . . . . .	.87	.18	.21
6. Substitution . . . . .	1.60	.21	.13
7. Number-group, cancel 3 . . . . .	.79	.06	.08
Number-group, cancel 28 . . . . .	1.14	.10	.09

To return now to the problem of discovering a method of combining the individual's records in several tests: There is a way of eliminating both of the troublesome quantities—both the absolute value of the average and the absolute measure of variability. Let the average in each case be counted as 0, *i. e.*, let the individual's standing be expressed as a *deviation* above or below the average; and further, let the measure of variability be taken as the unit deviation, and all deviations be expressed as fractions or multiples of this unit. (For the measure of variability, either the average deviation, or the

<sup>1</sup> For the tests named in the table, see Woodworth and Wells, *Association Tests*, *PSYCHOLOGICAL REVIEW*, Monogr. Suppl., 1911. The number of individuals tested in the above series, thirteen, was too small for final certainty.

mean square deviation, or the quartile, etc., may be chosen.) What this method does is to assign each individual's *position in the distribution* of the group: he stands, namely, above or below the group average, and so and so much above or below as compared with the average variation of the group.

No assumption is made by this method as to the ratio between the variability and the group average; for the average is taken as 0 and the variability as 1, independently the one of the other. The only assumptions underlying the method are those involved in every use of averages and variabilities, namely, that the average means the same thing in respect to one distribution as in respect to another, and, likewise, that the measure of variability means the same thing in respect to the different distributions. Both of the assumptions are correct if the distributions are of the 'normal' type, or if all the distributions belong to any one type. Were one distribution normal, another markedly skew, and a third distinctly bimodal, neither the average nor the average deviation would mean quite the same thing in respect to the three, and the method would be illegitimate; but in such a case it is doubtful if the distributions ought properly to be combined at all. Mental tests usually give group distributions not very different from the 'normal,' though tending on the whole to be somewhat skew in such a way that more individuals lie on the good side of the average than on the bad side. The distributions for different tests do not differ much in shape, and no considerable error can be introduced by placing the average always equal to 0 and the average deviation (or mean square deviation, etc.) always equal to 1.<sup>1</sup>

When the individual's position in each single distribution

<sup>1</sup> To repeat: this procedure involves no assumptions that are not involved in the ordinary statistical operations with averages, average deviations and coefficients of correlation. It is simply assumed to be fair to compare the individual with the group average as a standard, and to measure his deviation from the standard in terms of the group variability, and, further, to compare the results so obtained in different tests. If the average means anything that is constant for all the distributions, and if the average deviation means anything that is the same for all the distributions, then the assumptions are justified. It is true, of course, that the method always measures the individual by his relations to the group, and that, for some purposes, absolute and not relative measures are what is required.



has thus been determined, his *average position* in two or more distributions can be got, as well as the variability of his positions. If he stands  $+ .8$ ,  $+ .4$ , and  $- .3$  in three tests, his average standing is  $+ .3$ , and the a.d. of his position is  $.4$ . If one asks, "Four-tenths of what?" the answer is that *the unit is throughout the variability of the group in the single test*.

The method and its utility will now be illustrated by the results of a set of association tests. Thirteen college and university students (eight men and five women) were examined with nine rather similar tests, called the 'logical relations' tests. The results of each single test were first treated by themselves: the average time of the thirteen subjects was found, and the  $+$  or  $-$  deviation of each individual. The mean square deviation was then found and used as the unit, all the single deviations being divided by it and expressed as per cents of it. The individuals who did better than the average were marked  $+$  and those who did worse than the average were marked  $-$ . In the accompanying table, each vertical column contains the standings of the same individual in the different tests.  $F_1, F_2$ , etc., are the women, and  $M_1, M_2$ , etc., the men. The individuals are arranged, from left to right, in the order of their average proficiency in these tests. Below the single records of each individual is a number in heavy type giving his average standing, and below this, again, is a measure of his variability of standing. (In this table, the mean square deviation,  $\sigma$ , has been consistently used as the measure of variability. The relative advantages of this measure and of the a.d. will be discussed later on.)

Inspection of the table shows at once that a high positive correlation is to be expected, and it further shows certain facts regarding the individuals which would be lost if only coefficients of correlation were computed. For example, the individual  $F_1$  consistently occupies a very high position in all the tests, and the positive correlations are due to this individual's consistency more than to any other individual, though there are a few who are nearly as consistent in occupying a low position, and one or two who are fairly consistent in occupying a middle position. There are, on the other hand, individuals whose

STANDINGS OF 13 INDIVIDUALS IN THE LOGICAL RELATIONS TESTS

$$Unit = \frac{\sigma}{100}$$

Individuals.	$F_1$	$M_1$	$F_2$	$M_2$	$M_3$	$M_4$	$F_3$	$M_5$	$M_6$	$F_4$	$F_5$	$M_7$	$M_8$
<i>Tests</i>													
Opposites.....	+159	+58	+131	+117	+6	-43	+37	+18	-117	-124	-7	-80	-177
Verb-object.....	+172	+71	+73	+41	+17	+46	+106	-124	-95	-52	-97	-190	+28
Subord. concept.....	+189	+152	-41	+21	-8	+111	-115	-	+37	-98	-94	+8	-152
Supraord. concept.....	+197	+135	+94	-4	+21	+8	-16	+29	-37	-12	-172	-131	-131
Part-whole.....	+192	+20	+104	+44	+16	-44	+12	+84	+36	-84	-92	-76	-224
Whole-part.....	+148	+53	+95	+111	+5	-21	+127	+27	-48	-164	-143	-117	-64
Agent-action.....	+192	+110	+89	+75	+7	-62	-110	+55	+7	-82	-21	-185	-82
Action-agent.....	+152	+72	+104	+32	+52	+24	-52	-60	-56	+44	-36	-4	-268
Attribute-substance.....	+155	+125	+77	+5	+53	+58	-48	-259	-43	+24	+53	+38	-55
Average standing in logical relations (i) ...	+173	+88	+81	+49	+19	-4	-7	-26	-35	-66	-68	-82	-125
Variability of individual's standing in logical relations ( $\sigma t$ ).....	18	42	45	44	20	53	81	101	50	60	67	78	87

standing varies much from test to test. This individual difference in consistency finds an expression in the variability of the individual's standing,  $\sigma_i$ , the mean square deviation of the individual's standing. In no other way, probably, except by the method here employed, would it be possible to reach a measure of this sort of personal characteristic, though it is a characteristic that impresses the experimenter as he conducts subjects through a series of tests. Of some individuals he would, after experience with them, be greatly surprised to see them do badly; of others he would be surprised to find them doing well; while of still others he would not be surprised at anything. These impressions of the experimenter are confirmed and made exact by the measure here given.

When the average standing is compared with the individual variability, it is seen that the individuals who stand high are more consistent than those who stand low. The Pearson coefficient of correlation between the standing and the consistency is .72. Some individuals with a middle average standing have, indeed, very high variabilities; and this would be expected, since any individual who does vary greatly from good to bad will tend toward a middle average position. It might be expected that the individuals with the lowest standing would be as consistent in their inferiority as the best individuals were in their superiority; but this is not true within the range of individuals here examined; for, whereas the best individuals suffered no lapse towards mediocrity, the worst individuals sometimes gave an unexpected exhibition of efficiency.

## II

Since the labor involved in the above method is considerable, it may be well to show that, once this labor is performed, the operation of computing coefficients of correlation is considerably simplified; so that, if the investigator desires to measure the average correlation, he is compensated therein to some degree for the time spent in obtaining the average individual standings. In addition, he is likely to gain a more precise insight into the causes of correlation from being able to trace the individual contribution to the average result, and



from being able to obtain, besides the Pearson coefficient, certain other measures of correlation which are in some respects more easily understood.

What the writer here attempts is to derive *simplified formulæ*, available when the original measures have been reduced to terms of the a.d. or the mean square deviation, for (1) the Pearson coefficient of correlation between two tests; (2) the average correlation within any number of tests, as measured by Boas;<sup>1</sup> and (3) the Spearman correction for attenuation.

In the mathematical considerations which follow, I shall first assume that the original measures have been reduced to terms of the mean square deviation ( $\sigma$ ), and later shall consider what modifications are required if the reduction has been to terms of the a.d.; and shall finally consider the relative advantages of the two reductions.

Consider first two tests,  $A$  and  $B$ , with each of which a measure is obtained of the individuals 1, 2, 3, . . . . Let the original measures be reduced, as above explained, to deviations divided by the mean square deviation in each test; and let  $a_1$  be the reduced measure of individual 1 in test  $A$ , etc. Then the two series of measures may be represented thus:

$$\begin{array}{ll} a_1 & a_2 & a_3 & a_4 & a_5 & \dots & \Sigma a = 0; & \text{Av } a^2 = 1 \\ b_1 & b_2 & b_3 & b_4 & b_5 & \dots & \Sigma b = 0; & \text{Av } b^2 = 1. \end{array}$$

$\Sigma a = 0$ ; i. e., the algebraic sum of the deviations is 0, because the sum of the positive deviations from the average = the sum of the negative deviations.  $\text{Av } a^2 = 1$ ; i. e., the mean square deviation = 1, being taken as the unit.<sup>2</sup>

Now let the average standing of each individual be found, and represented by  $s_1, s_2, s_3, \dots$ , in which

$$s_1 = \frac{a_1 + b_1}{2}, \quad s_2 = \frac{a_2 + b_2}{2}, \text{ etc.}$$

<sup>1</sup> *Science*, 1909, 29, 824.

<sup>2</sup> The use of the symbols  $\Sigma$  and  $\text{Av}$  in what follows will be sufficiently obvious. The main points are that they can be distributed to the several terms in a sum (i. e.,  $\Sigma(a + b + c) = \Sigma a + \Sigma b + \Sigma c$ , and  $\text{Av}(a + b + c) = \text{Av } a + \text{Av } b + \text{Av } c$ , and that a constant factor can be freely moved from one side to the other of the symbol (i. e., if  $p$  is constant,  $\Sigma pa = p\Sigma a$ ; and  $\text{Av } pa = p \text{Av } a$ ; also, of course,  $\text{Av } p = p$ ).

Then

$$\begin{aligned}\Sigma s &= \Sigma \frac{a+b}{2} \\ &= \frac{1}{2}(\Sigma a + \Sigma b) \\ &= 0,\end{aligned}\tag{1}$$

since both  $\Sigma a$  and  $\Sigma b$  are 0. That is to say that the  $s$ 's are deviations from the center of the average distribution; so that the variability of the  $s$ 's, or  $\sigma_s$ , is given by the equation,

$$\sigma_s^2 = \text{Av } s^2.\tag{2}$$

We wish now to find the coefficient of correlation between  $A$  and  $B$ . The well-known formula for the Pearson coefficient is

$$r = \frac{\Sigma ab}{n\sigma_a\sigma_b},$$

in which  $a$  and  $b$  are deviations. Here, however, both  $\sigma_a$  and  $\sigma_b$  have been taken as units, and therefore,

$$\begin{aligned}r &= \frac{\Sigma ab}{n} \\ &= \text{Av } ab.\end{aligned}\tag{3}$$

In words, the coefficient of correlation is the average product of the reduced measures of an individual.

Another expression for the coefficient has some advantages. It is obtained as follows:

$$\begin{aligned}\text{Av } s^2 &= \text{Av } \left( \frac{a+b}{2} \right)^2 \\ &= \frac{1}{4} \text{Av } (a+b)^2 \\ &= \frac{1}{4} \text{Av } (a^2 + b^2 + 2ab) \\ &= \frac{1}{4} (\text{Av } a^2 + \text{Av } b^2 + 2 \text{Av } ab) \\ &= \frac{1}{4} (1 + 1 + 2r) \\ &= \frac{1}{2} (1 + r).\end{aligned}$$

And therefore, by transposition,

$$r = 2 \text{Av } s^2 - 1,$$

or,

$$r = 2\sigma_s^2 - 1.\tag{4}$$

If the  $s$ 's have already been found, in order to know each individual's average standing, then the  $r$  can be more easily obtained by this formula than from  $\text{Av } ab$ .

Consider next the case in which more than two tests are in question. As before, let the individuals be designated 1, 2, 3, . . . , the whole number being denoted by  $n$ ; and let the tests be designated  $A, B, C, \dots$ , the number of tests being  $m$ . Then a table of the reduced results takes the form

$a_1$	$a_2$	$a_3$	$a_4$	. . .	$\Sigma a = 0; \quad \text{Av } a^2 = 1$
$b_1$	$b_2$	$b_3$	$b_4$	. . .	$\Sigma b = 0; \quad \text{Av } b^2 = 1$
$c_1$	$c_2$	$c_3$	$c_4$	. . .	$\Sigma c = 0; \quad \text{Av } c^2 = 1$
.	.	.	.	. . .	. . . . .
.	.	.	.	. . .	. . . . .
$s_1$	$s_2$	$s_3$	$s_4$	. . .	$\Sigma s = 0; \quad \sigma_s^2 = \text{Av } s^2$

$$s_1 = \frac{1}{m} (a_1 + b_1 + c_1 + \dots), \text{ etc.,}$$

from which it is evident, as above in the case of two tests, that  $\Sigma s = 0$ ; and hence the  $s$ 's are their own deviations, and

$$\sigma_s^2 = \text{Av } s^2. \quad (5)$$

To find a measure of the mutual agreement of all the tests, we may find the average of the coefficients of correlation between the pairs of tests. This can be got in terms of  $\text{Av } s^2$ , as follows:

$$\begin{aligned} \text{Av } s^2 &= \text{Av} \left( \frac{a + b + c + \dots}{m} \right)^2 \\ &= \frac{1}{m^2} \text{Av} (a + b + c + \dots)^2 \\ &= \frac{1}{m^2} (\text{Av } a^2 + \text{Av } b^2 + \text{Av } c^2 + \dots + 2 \text{Av } ab + 2 \text{Av } ac \\ &\quad + 2 \text{Av } bc + \dots). \end{aligned}$$

But this expression can be much simplified, for  $\text{Av } a^2 = 1$ ,  $\text{Av } b^2 = 1$ , etc., there being  $m$  of these terms that reduce to 1. Moreover,  $\text{Av } ab = r_{a,b}$ ,  $\text{Av } ac = r_{a,c}$ , etc.; and there are similar terms for all the  $r$ 's. Making these substitutions, we have

$$\text{Av } s^2 = \frac{1}{m^2} [m + 2\Sigma r].$$



Now we desire the average  $r$ . The number of  $r$ 's is equal to the number of ways in which  $m$  tests can be compared two at a time, or  $\frac{m(m-1)}{2}$ , and therefore

$$\Sigma r = \frac{m(m-1)}{2} \text{Av } r.$$

Making this substitution, we have

$$\begin{aligned} \text{Av } s^2 &= \frac{1}{m^2} [m + m(m-1) \text{Av } r] \\ &= \frac{1}{m} [1 + (m-1) \text{Av } r]. \end{aligned}$$

Therefore, finally,

$$\text{Av } r = \frac{m \text{Av } s^2 - 1}{m - 1}, \quad (6)$$

or,

$$\text{Av } r = \frac{m\sigma_s^2 - 1}{m - 1}. \quad (7)$$

Once the average standings of the individuals are found, therefore, very little further work is necessary to calculate the average coefficient of correlation of the whole series of tests.

When  $m = 2$ , this expression reduces to that already given for the correlation between two series, viz.,

$$r = 2 \text{Av } s^2 - 1 = 2\sigma_s^2 - 1.$$

Since  $\text{Av } r$  and  $\text{Av } s^2$  are so simply related to each other, it is evident that either might stand as a measure of the agreement of the set of tests. It will be worth while to dwell on these two measures for a moment.

Since the maximum value of any  $r$  is 1, the maximum value of  $\text{Av } r$  is 1, and in this case the equation (6),

$$\text{Av } r = \frac{m \text{Av } s^2 - 1}{m - 1},$$

shows that  $\text{Av } s^2 = 1$ . This then is the condition for perfect correlation between all the tests. That this is so is also evident directly, for perfect correlation means that each indi-

vidual has the same standing in any one test as he has in any other; and therefore his average standing,  $s$ , is the same as his standing in any test, such as  $a$ . Accordingly,

$$\text{Av } s^2 = \text{Av } a^2 = 1.$$

Or, otherwise expressed, when correlation is perfect,

$$\sigma_s = 1$$

or, the variability of the average distribution is equal to the variability of any single distribution. Anything less than perfect correlation causes the average distribution to fall together, and the variability of the average distribution to be less than 1, *i. e.*, less than that of the single distribution. The minimum value of  $\text{Av } s^2$  (since the square of a real number cannot be negative) is 0; and  $\text{Av } s^2$  could not be 0 unless each and every  $s$  were 0, which would mean that all the individuals were equal in the average distribution. This is obvious when the number of tests is only two, for then nothing would reduce all the individuals to equality in the average, except complete compensation or opposition between the results of the two tests. The formula shows the same, for, when  $m = 2$ , and  $\text{Av } s^2 = 0$ ,  $r = -1$ . In general the minimum value of  $\text{Av } r$  is found by putting  $\text{Av } s^2 = 0$  in the above equation, whence

$$\text{Av } r = -\frac{1}{m-1}.$$

When  $m = 2$ , this gives  $\text{Av } r = -1$ ; when  $m = 3$ ,  $\text{Av } r = -\frac{1}{2}$ , when  $m = 4$ ,  $\text{Av } r = -\frac{1}{3}$ , etc., in a series of negative values tending toward 0 as the number of tests increases. This is vaguely obvious to common sense, for not more than two sets of measures can be in complete opposition, each to each.

In some respects,  $\sigma_s$  gives a more intelligible measure of the agreement of several tests than is afforded by the more familiar  $\text{Av } r$ . It shows how much the variability of the average distribution is lessened in comparison with the variability of the single distribution. It ranges between the extremes 1 and 0, no matter what the number of tests; 1

always signifying perfect agreement and 0 the most complete disagreement possible; whereas the lower limit of  $\text{Av } r$  rises with the number of tests. On the other hand,  $\text{Av } r$  has the advantage of always being 0 for a chance relation of the several tests, while the value of  $\sigma_s$ , corresponding to  $\text{Av } r = 0$ , changes with the number of tests. If  $\text{Av } r$  is made 0, the formula (7) shows that

$$\sigma_s^2 = \frac{1}{m},$$

or,

$$\sigma_s = \sqrt{\frac{1}{m}}$$

When  $m = 2$ , then,

$$\sigma_s = \sqrt{\frac{1}{2}} = .71;$$

when  $m = 3$ ,  $\sigma_s = .57$ ; when  $m = 4$ ,  $\sigma_s = .50$ , etc., for chance correlation.

Still another measure of correlation deserves brief mention. Evidently, perfect correlation requires that each individual shall maintain always the same position in the several distributions; his variability of standing must be 0. As the individual's variability increases, the agreement between the tests decreases; and the maximum of disagreement would require that the individual's position in the different distributions should vary, on the average, as much as the positions of the different individuals in the single test.

An expression for  $\text{Av } \sigma_i^2$  would thus be a measure of the average consistency of the individuals, and so of the correlation of all the tests.

Consider again the symbolic table

$a_1$	$a_2$	$a_3$	$a_4$	. . .
$b_1$	$b_2$	$b_3$	$b_4$	. . .
$c_1$	$c_2$	$c_3$	$c_4$	. . .
.	.	.	.	. . .
.	.	.	.	. . .
<hr/>				
$s_1$	$s_2$	$s_3$	$s_4$	. . .
$\sigma_1$	$\sigma_2$	$\sigma_3$	$\sigma_4$	



The symbol  $\sigma_1$  is used for the mean square deviation of individual 1 about his average standing  $s_1$ . His single deviations from this average are  $s_1 - a_1, s_1 - b_1, s_1 - c_1, \dots$ , and therefore,

$$\begin{aligned}\sigma_1^2 &= \frac{1}{m} [(s_1 - a_1)^2 + (s_1 - b_1)^2 + (s_1 - c_1)^2 + \dots] \\ &= \frac{1}{m} [s_1^2 - 2s_1a_1 + a_1^2 \\ &\quad + s_1^2 - 2s_1b_1 + b_1^2 \\ &\quad + s_1^2 - 2s_1c_1 + c_1^2 \\ &\quad + \dots] \\ &= \frac{1}{m} [ms_1^2 - 2s_1(a_1 + b_1 + c_1 + \dots) + (a_1^2 + b_1^2 + c_1^2 + \dots)] \\ &= s_1^2 - 2s_1 \frac{a_1 + b_1 + c_1 + \dots}{m} + \frac{1}{m} (a_1^2 + b_1^2 + c_1^2 + \dots) \\ &= -s_1^2 + \frac{1}{m} (a_1^2 + b_1^2 + c_1^2 + \dots),\end{aligned}$$

since

$$\frac{a_1 + b_1 + c_1 + \dots}{m} = s_1.$$

This gives the  $\sigma_i^2$  for one individual; and if it is found for each of the  $n$  individuals, and the results are all added together and divided by  $n$ , we get

$$\begin{aligned}\text{Av } \sigma_i^2 &= -\text{Av } s^2 + \frac{1}{mn} [a_1^2 + b_1^2 + c_1^2 + \dots \\ &\quad + a_2^2 + b_2^2 + c_2^2 + \dots \\ &\quad + a_3^2 + b_3^2 + c_3^2 + \dots \\ &\quad + \dots] \\ &= -\text{Av } s^2 + \frac{1}{mn} [\Sigma a^2 + \Sigma b^2 + \Sigma c^2 + \dots] \\ &= -\text{Av } s^2 + \frac{1}{m} [\text{Av } a^2 + \text{Av } b^2 + \text{Av } c^2 + \dots] \\ &= -\text{Av } s^2 + \frac{1}{m} [1 + 1 + 1 + \dots] \\ &= -\text{Av } s^2 + 1,\end{aligned}$$

since there are  $m$  of the terms that reduce to 1. Thus,

$$\text{Av } \sigma_i^2 + \text{Av } s^2 = 1,$$

or,

$$\text{Av } \sigma_i^2 + \sigma_s^2 = 1. \quad (8)$$

This equation, along with the equation,  $\text{Av } s = 0$ , affords a good check on the accuracy of the computations.

The demonstration of the above equation can be put into a more compact form by employing the letter  $x$  to represent any one of the letters  $a, b, c, \dots$ , and by using  $\text{Av}_m$  to indicate the average of  $m$  quantities corresponding to the  $m$  tests, and  $\text{Av}_n$  to indicate the average of  $n$  quantities corresponding to the  $n$  individuals. Then, for any one individual, we have

$$\begin{aligned} \sigma_i^2 &= \text{Av}_m (s - x)^2 \\ &= \text{Av}_m s^2 - 2s \text{Av}_m x + \text{Av}_m x^2 \\ &= s^2 - 2s^2 + \text{Av}_m x^2 \\ &= -s^2 + \text{Av}_m x^2. \end{aligned}$$

And, for all the individuals, we have

$$\begin{aligned} \text{Av}_n \sigma_i^2 &= -\text{Av}_n s^2 + \text{Av}_{mn} x^2 \\ &= -\text{Av}_n s^2 + 1, \end{aligned}$$

as before.

A demonstration very similar to this last shows another interesting property of  $\sigma_s$ . Sometimes it is desired to correlate the standing of the individuals in a single test with their average standing in all the tests; for if the correlation is high, the single test is indicated as being a good measure of the whole set of similar performances. If the coefficient of correlation between a single test,  $A$ , and the average of all be designated by  $r_{a, s}$ , then

$$\begin{aligned} r_{a, s} &= \frac{\text{Av}_n a s}{\sigma_a \sigma_s} \\ &= \frac{\text{Av}_n a s}{\sigma_s}, \end{aligned}$$

since  $\sigma_a = 1$ . If each of the  $m$  tests,  $A, B, C, \dots$ , be corre-

lated with the average of all, and if  $x$  be used to indicate any one of the letters  $a, b, c, \dots$ , then

$$\begin{aligned} \text{Av}_m r_{x, s} &= \text{Av}_m \left( \frac{\text{Av}_n x s}{\sigma_s} \right) \\ &= \frac{1}{\sigma_s} \text{Av}_{mn} x s \\ &= \frac{1}{\sigma_s} \text{Av}_n \text{Av}_m x s \\ &= \frac{1}{\sigma_s} \text{Av}_n (s \text{Av}_m x) \end{aligned}$$

(since, for any column of  $m$  numbers, as  $a_1, b_1, c_1, \dots$ , the corresponding  $s_1$  is constant)

$$= \frac{1}{\sigma_s} \text{Av}_n s^2$$

(since  $\text{Av}_m x =$  the average of the  $m$  numbers in one column, as  $a_1, b_1, c_1, \dots$ , and the corresponding  $s_1$  is this average)

$$\begin{aligned} &= \frac{\sigma_s^2}{\sigma_s} \\ &= \sigma_s. \end{aligned} \tag{9}$$

Therefore  $\sigma_s$  is the average value of the coefficients obtained by correlating the average standings with the standings in each single test.<sup>1</sup>

<sup>1</sup> It is instructive to apply such an equation to special cases. When the number of tests is 2, we have the equations, (4) and (9),

$$\begin{aligned} r_{x, y} &= 2\sigma_s^2 - 1; \\ \text{Av } r_{x, s} &= \sigma_s. \end{aligned}$$

Whence

$$\text{Av } r_{x, s} = \sqrt{\frac{r_{x, y} + 1}{2}}.$$

If, then,  $r_{x, y} = 0$ , i. e., if the two tests have only a chance relation with each other, then the average standing in the two correlates with either one according to the coefficient,

$$r_{x, s} = \sqrt{\frac{1}{2}} = +.71.$$

This affords a rather tangible notion of the degree of correspondence indicated by the coefficient  $+.71$ . When two series of measurements give the coefficient  $+.71$ , the agreement between them is the same as that obtaining between a series of measurements and the average of itself with a totally unrelated series. Or, one might almost say, if two wholly independent factors contribute equally to a variable result, the result gives a coefficient of  $+.71$  with that which would be given by either factor acting alone.



Now for the Spearman correction for attenuation. If  $A$  and  $B$  are two tests, presumed to be tests of the same function, and  $X$  and  $Y$  are two tests of another function, then Spearman gives the following formula for the true coefficient of correlation between the two functions:

$$r = \frac{\frac{1}{4}(r_{a,x} + r_{a,y} + r_{b,x} + r_{b,y})}{\sqrt{r_{a,b} \cdot r_{x,y}}}.$$

That is to say, we are required to find the Pearson coefficients between  $A$  and  $X$ , between  $A$  and  $Y$ , between  $B$  and  $X$ , and between  $B$  and  $Y$ ; and also those between  $A$  and  $B$  and between  $X$  and  $Y$ ; and then divide the arithmetical mean of the first four by the geometrical mean of the last two. This rather laborious process can be much abridged provided the results of each test have been for other reasons reduced to terms of  $\sigma$ , and if also the average position of each individual in the pairs of tests,  $A$  and  $B$ , and  $X$  and  $Y$ , has been determined. The symbol  $s_{a,b}$  may be employed to denote the average standing of any individual in the two tests  $A$  and  $B$ . Spearman's formula reduces as follows:

$$\begin{aligned} r &= \frac{\frac{1}{4}(r_{a,x} + r_{a,y} + r_{b,x} + r_{b,y})}{\sqrt{r_{a,b} \cdot r_{x,y}}} \\ &= \frac{\frac{1}{4}(\text{Av } ax + \text{Av } ay + \text{Av } bx + \text{Av } by)}{\sqrt{r_{a,b} \cdot r_{x,y}}} \\ &= \frac{\frac{1}{4}\text{Av}(ax + ay + bx + by)}{\sqrt{r_{a,b} \cdot r_{x,y}}} \\ &= \frac{\frac{1}{4}\text{Av}(a + b)(x + y)}{\sqrt{r_{a,b} \cdot r_{x,y}}} \\ &= \frac{\text{Av}\left(\frac{a + b}{2}\right)\left(\frac{x + y}{2}\right)}{\sqrt{r_{a,b} \cdot r_{x,y}}} \\ &= \frac{\text{Av } s_{a,b} s_{x,y}}{\sqrt{r_{a,b} \cdot r_{x,y}}}. \end{aligned} \tag{10}$$

We have therefore to proceed as follows: Find the standing of each individual (in terms of  $\sigma$ ) in test  $A$  and in test  $B$ ,

and his average standing in these two, and also the correlation between these two; likewise his standing in  $X$ , in  $Y$ , and his average standing in these two, as well as the correlation between them. Then multiply each individual's average standing in  $A$  and  $B$  by his average standing in  $X$  and  $Y$ , and divide as indicated in the formula.

The preceding formulæ have been based on the reduction of the original measures to terms of  $\sigma$ , and it remains to be seen whether the formulæ can be adapted to use when measurements are reduced to terms of a.d. This can easily be accomplished on the assumption of a constant relation between  $\sigma$  and a.d. Theoretically,  $\sigma = 1.2533$  a.d., *i. e.*, this is the ratio between the two measures in the normal distribution. This ratio will seldom be found exactly realized when a.d. and  $\sigma$  are calculated from short series of measurements; partly because the distributions are not strictly normal, and partly because neither  $\sigma$  nor a.d., as calculated from a short series of measurements, exactly represents what would be the  $\sigma$  or a.d. if the number of measurements were sufficiently large. As between these two measures of variability, preference is given by statisticians to  $\sigma$ , because its true value is usually more closely approximated in a small series of measurements. On the other hand, psychologists have often expressed a preference for a.d., because it is less affected by the extreme accidental variations that frequently occur in mental performances. In mental tests, a.d. is subject to frequent small errors, and  $\sigma$  to occasional large errors.

When expressed in terms of a.d., each individual's standing will evidently have the same sign as when expressed in terms of  $\sigma$ , and will simply be numerically larger in the ratio of  $\sigma$  to a.d. Each individual's standing will therefore be expressed by a number 1.2533 times as large (theoretically) as when  $\sigma$  is used as unit. (Or, if any other measure of variability, such as the quartile, were used as the unit, the reduced measures in any series would have a constant ratio to those obtained with  $\sigma$  as unit.) If, for convenience, the letter  $p$  be used to represent the ratio of  $\sigma$  to a.d. (or other unit

employed), and if  $a_1, a_2, a_3, \dots$  and  $b_1, b_2, b_3, \dots$  be used, as before, to designate the deviations expressed in terms of  $\sigma$ , then  $pa_1, pa_2, pa_3, \dots$ , and  $pb_1, pb_2, pb_3, \dots$  will be the measures expressed in terms of a.d.

$$\begin{aligned}\text{Now} \quad r_{a,b} &= \text{Av } ab \\ &= \frac{1}{p^2} \text{Av } (pa)(pb).\end{aligned}$$

When, therefore, the unit in each series is the a.d., the coefficient of correlation is found by taking the average product of an individual's marks, and dividing by  $(1.2533)^2$ , or, what is the same thing, multiplying by 0.64 (two places being all the accuracy the whole method can stand).

This way of finding  $r$  is not, however, to be recommended, for the reason that, when  $p$  is actually different from the theoretical value, the value of  $r$  does not have its accustomed limits. Thus, if  $p$  equals, in each of two given series of measurements, 1.10 instead of 1.25, the maximum value of  $r$ , for perfect correlation, is  $+.78$  instead of the customary  $+1.00$ ; and if  $p = 1.40$ , the maximum value of  $r = +1.26$ . Since the utility of  $r$  is dependent on the constancy of its limits, such irregularities would spoil it.

The following considerations do not depend on the absolute value of the ratio between  $\sigma$  and a.d., though they do assume the constancy of the ratio. After the formulæ have been derived, consideration will be given to the inaccuracy introduced when the ratio is not constant. The ratio is still represented by  $p$ .

Let individuals 1, 2, 3,  $\dots$  be measured by tests  $A, B, C, \dots$ , and let the results be reduced, first to terms of  $\sigma$ , and again to terms of a.d., and let tables prepared by the two methods be set down side by side; thus, symbolically:

In terms of  $\sigma$

$a_1$	$a_2$	$a_3$	$a_4$	$\dots$
$b_1$	$b_2$	$b_3$	$b_4$	$\dots$
$c_1$	$c_2$	$c_3$	$c_4$	$\dots$
$\dots$	$\dots$	$\dots$	$\dots$	$\dots$
$\dots$	$\dots$	$\dots$	$\dots$	$\dots$
$s_1$	$s_2$	$s_3$	$s_4$	$\dots$

In terms of a.d.

$pa_1$	$pa_2$	$pa_3$	$pa_4$	$\dots$
$pb_1$	$pb_2$	$p'b_3$	$pb_4$	$\dots$
$pc_1$	$pc_2$	$pc_3$	$pc_4$	$\dots$
$\dots$	$\dots$	$\dots$	$\dots$	$\dots$
$\dots$	$\dots$	$\dots$	$\dots$	$\dots$
$ps_1$	$ps_2$	$ps_3$	$ps_4$	$\dots$



Since, as previously shown,  $\Sigma s = 0$ ,

$$\Sigma ps = p\Sigma s = 0, \quad (11)$$

and therefore,  $ps_1, ps_2, ps_3, \dots$  are deviations from the center of the average distribution. Accordingly the numerical average of these deviations is the a.d. of the average distribution, or a.d.<sub>ps</sub>. Now

$$\text{a.d.}_{ps} = p \times \text{a.d.}_s$$

But, since the ratio of a.d. to  $\sigma$  is (theoretically) still the same in this average distribution as in any other,

$$\text{a.d.}_s = \frac{1}{p} \sigma_s,$$

and, therefore,

$$\text{a.d.}_{ps} = p \times \text{a.d.}_s = p \times \frac{1}{p} \sigma_s = \sigma_s. \quad (12)$$

In other words, if, in one case, the  $\sigma$  of the single distribution is taken as unit, and in another case the a.d. of the single distribution is taken as unit, the numerical values of  $\sigma$  of the average distribution in the first case and of a.d. of the average distribution in the second case will be the same. Or, again, a.d. of the average distribution has the same ratio to a.d. of the single distribution as  $\sigma$  of the average distribution has to  $\sigma$  of the single distribution. This seems rather obvious, but, as will be shown later, it is not always true, and some difficulty in the use of the following formulæ may arise when it is not true.

Since, then,  $\text{a.d.}_{ps} = \sigma_s$ , we can substitute in the previous formula (7) for the average coefficient of correlation, and have, instead of

$$\text{Av } r = \frac{m\sigma_s^2 - 1}{m - 1},$$

the following formula, to be used when the data of each test are expressed in terms of a.d. as unit:

$$\text{Av } r = \frac{m(\text{a.d.}_{ps})^2 - 1}{m - 1}.$$

The  $p$  in the subscript of a.d.<sub>ps</sub>, being of use only to distinguish the procedure with a.d. as the unit from the procedure with

$\sigma$  as the unit, can be dropped from the final formula, with the understanding that the formula is only to be used when a.d. is the unit throughout; then we have

$$Av\ r = \frac{m\ a.d._s^2 - 1}{m - 1}. \quad (13)$$

What was said before of the use of  $\sigma_s$  as a measure of correlation evidently applies also to a.d.<sub>s</sub>. The value of a.d.<sub>s</sub> is 1 for perfect agreement between all the tests, and 0 for the most complete disagreement possible.

When the number of tests, or  $m$ , is 2, the above formula reduces to the following:

$$r = 2\ a.d._s^2 - 1. \quad (14)$$

This gives a quick way of computing  $r$ , and is worthy to be regarded as one of the best abridged or 'foot-rule' methods of finding the Pearson coefficient. Its use may be illustrated in an actual case.

When the results of two color naming tests on 13 individuals are expressed in terms of a.d., they appear as in the accompanying table. The line beginning  $s$  gives each individual's average standing, and the numerical average of the  $s$ 's gives a.d.<sub>s</sub>. Here a.d.<sub>s</sub> = .96, and the formula,

$$r = 2\ a.d._s^2 - 1,$$

gives  $r = +.84$ . Calculated from the regular Pearson formula,  $r = +.83$ . Such close agreement as this is not always to be expected, but in my experience the agreement is usually close.<sup>1</sup>

<sup>1</sup> This method can be still further abridged, for since the average of the numbers in the first line = 1 (disregarding signs), and likewise the average of the numbers in the second line = 1, the average of them all (still disregarding signs) must = 1. Now this average of all would be a.d.<sub>s</sub>, except for the fact that in finding each  $s$  the signs are considered. If therefore the signs had been disregarded in finding the  $s$ 's, a.d.<sub>s</sub> would have been = 1. It is only the cases of unlike signs that diminish the value of a.d.<sub>s</sub>. In the table,  $M_2$  and  $M_6$  are the only cases that cause diminution of a.d.<sub>s</sub>. If, in these two cases, the arithmetical sums of 29 and 75, and of 24 and 75, had been taken, instead of the arithmetical differences, a.d.<sub>s</sub> would have been equal to 1. Take the case of  $M_2$ . By taking the difference of 29 and 75, instead of their sum, the corresponding  $s$  is made less by 29 than it would have been; and, in any case, the value of  $s$  is diminished by the amount of the smaller of the two numbers which have unlike

RESULTS OF THE COLOR-NAMING TESTS, EXPRESSED IN TERMS OF A.D.

Individuals	$F_1$	$M_1$	$F_2$	$M_2$	$M_3$	$M_4$
Test 1.....	+187	+108	+160	+29	-116	+ 3
Test 2.....	+ 85	+ 57	+160	-75	-123	+113
$\bar{s}$ .....	+136	+ 82	+160	-23	-119	+ 58

Individuals	$F_3$	$M_5$	$M_6$	$F_4$	$F_5$	$M_7$	$M_8$
Test 1.....	+160	- 24	-24	+3	-155	-24	-313
Test 2.....	+151	-113	+75	0	- 19	-38	-283
$\bar{s}$ .....	+155	- 68	+25	+1	- 87	-31	-298

That other measure of correlation, the  $Av \sigma_i^2$  is also available when a.d. has been chosen as unit. For consider the measures of any one individual in the two tables above. When  $\sigma$  is the unit, his measures are  $a_1, b_1, c_1, \dots$ ; and, when a.d. is the unit, they are  $pa_1, pb_1, pc_1, \dots$ . Then,

$$\text{a.d. of } (pa_1, pb_1, pc_1, \dots) = p \times \text{a.d. of } (a_1, b_1, c_1, \dots).$$

But

$$\text{a.d. of } (a_1, b_1, c_1, \dots) = \frac{1}{p} \times \sigma \text{ of } (a_1, b_1, c_1, \dots).$$

And therefore

$$\text{a.d. of } (pa_1, pb_1, pc_1, \dots) = \sigma \text{ of } (a_1, b_1, c_1, \dots).$$

In other words, the individual's variability of standing has the same numerical measure, whether  $\sigma$  or a.d. is used, consistently, signs. Therefore, to get a.d.s, we need only consider the cases of unlike signs; taking, in each such case, the smaller of the two numbers, finding the sum of these smaller numbers, and dividing by the whole number of individuals measured, we shall obtain the amount by which a.d.s. is less than 1. Thus, in the table, taking the smaller of the two numbers under  $M_2$  and  $M_6$ , we have, as their sum, 53, and dividing this by 13, the number of individuals, we have 4. Subtracting this from 1 (or 100 per cent.), we have a.d.s. = .96. Now if all that were desired from the results were the coefficient of correlation, we should not need to reduce every deviation to terms of a.d., but we might simply do this in the cases of unlike signs, and then deal with the smaller of the resulting numbers as above. When the correlation is fairly high, so that the number of unlike signs is small, this method of computing  $r$  is certainly to be called a quick method. Certain inaccuracies that may creep in will be discussed later, but it may be noted at once that, unless there are unlike signs, the formula gives  $r = 1$ , though the correlation may evidently be not quite perfect, and though the formulæ in terms of  $\sigma$  may give a coefficient as low as +.80. Any rearrangement of the numbers in the two lines of the table, provided such rearrangement did not increase the number of unlike signed pairs, would not change the coefficient of correlations. From this it can be seen that the method lacks something in delicacy, but it is only in rather exceptional cases that this lack of delicacy actually changes the value of  $r$ .



as the measure of variability.<sup>1</sup> Accordingly, in the previous equation (8),

$$Av \sigma_i^2 + \sigma_s^2 = 1,$$

we may substitute a.d.<sub>i</sub> for  $\sigma_i$ , as well as (see above) a.d.<sub>s</sub> for  $\sigma_s$ , and obtain the following equation for the case when a.d. is used throughout instead of  $\sigma$ :

$$Av (a.d._i)^2 + (a.d._s)^2 = 1. \quad (15)$$

In like manner, the formula given above for the Spearman corrected coefficient of correlation can be used when a.d. has been taken as the unit, just as if  $\sigma$  were the unit. If we take the formula,

$$\text{Spearman } r = \frac{Av (s_{a,b} s_{x,y})}{\sqrt{Av ab \cdot Av xy}},$$

and simply substitute the values for  $a, b, x, y, s_{ab}$ , and  $s_{xy}$ , which are got (see p. —) when a.d. is used as the unit, we have

$$\begin{aligned} & \frac{Av (p s_{a,b} \cdot p s_{x,y})}{\sqrt{Av (pa \cdot pb) \cdot Av (px \cdot py)}} \\ &= \frac{p^2 Av (s_{a,b} s_{x,y})}{\sqrt{p^2 Av ab \cdot p^2 Av xy}} \\ &= \frac{Av (s_{a,b} s_{x,y})}{\sqrt{Av ab \cdot Av xy}} \\ &= r. \end{aligned} \quad (16)$$

To summarize: the formulæ developed for measuring correlation when the original measurements have been reduced to terms of  $\sigma$  can also be used when the measurements have been reduced to terms of a.d. One need only substitute systematically a.d. wherever  $\sigma$  appears. The assumption underlying the formulæ in terms of a.d. is the constancy of the ratio,  $p$ , between  $\sigma$  and a.d. Note that the absolute value of  $p$  is not in question, but only its constancy. It remains to consider how far the formulæ are vitiated by the actual variability of  $p$ .

<sup>1</sup> The theoretical relation between a.d. and  $\sigma$  would not be expected to hold except where the number of cases (here the number of tests) was considerable. Only then should the above equation for a.d.<sub>i</sub> be used.

There are two ways in which error might creep in through inconstancy of  $p$ .

1. The value of  $p$  may change from one test to another, and, in fact, does vary, in short series of measurements, from about 1.10 to about 1.40. This might conceivably cause trouble by so affecting the relative values of the  $s$ 's that their distribution would be different according as  $\sigma$  or a.d. were used as the unit. When  $p$  varies from test to test, the values of  $s$  obtained in terms of a.d. need not be precisely proportional to those obtained in terms of  $\sigma$ . In practice, however, this source of error is negligible, first because the divergencies between the distribution of the  $s$ 's is always slight, and second because, where divergencies exist, the a.d. values *may* be the truer. Examination of the derivation of formula (16) shows that the only  $p$ 's there in question are those of the single tests and those expressing the ratio of the average standings, when a.d. has been the unit, to the average standings when  $\sigma$  has been the unit. Accordingly, this formula is not subject to any considerable error.

2. A more serious difficulty arises from the fact that  $p$  has been assumed to be constant, not only from one series of measurements to another, but also in the distribution of the average standings. It has been assumed, throughout the preceding deductions, that the ratio of  $\sigma$  to a.d. is the same in this average distribution as in the single distributions from which the average distribution is derived. Whether this is so or not depends on the nature of the correlation between the several series of measurements; and consideration of this matter brings to light an important point regarding correlations.

It has been shown above that the variability of the average distribution is less than that of the constituent single distributions, except when the correlation between the latter is perfect. Except in case of perfect positive correlation,  $\sigma_s$  is less than the  $\sigma$  of a single distribution (*i. e.*, less than 1, when  $\sigma$  of the single distribution is the unit), and a.d.<sub>s</sub> is less than a.d. of the single distribution. In other words, except in case of perfect positive correlation, individuals differ less from one another on the average of several tests than in any single test.

Now if this diminution of the variability affected all parts of the distribution alike, so that the average distribution had the same form as the single distributions, then the ratio of  $\sigma$  to a.d., depending as it does simply on the form of the distribution, would be the same in the average distribution as in the single tests. (Or, if  $p$  varied from one test to another, it should have, in the average distribution, a value not far from the average of its values in the single tests.) But it may happen, at least in short series of measurements, that the diminution of variability, on taking the average of the tests, does not affect all parts of the distribution equally. The writer has observed, in practice, two opposed cases:

(a) The individuals lying near the extremes of the distribution maintain their positions from one test to another, whereas the individuals near the center of the distribution shift about considerably from test to test. The average distribution then presents a few excessively large deviations along with many small ones; and its  $\sigma$  is accordingly very large in relation to its a.d. In other words,  $p$  in the average distribution is larger than in the constituent distributions. Of the two  $p$ 's which were assumed to cancel each other in deriving equation (12), that in the denominator is, under the present circumstances, larger than that in the numerator, and accordingly a.d.<sub>ps</sub> is actually smaller than  $\sigma_s$ ; and hence the coefficient of correlation, computed from formula (13) or (14), comes out smaller than from formula (7) or (4), or from the regular Pearson formula.<sup>1</sup>

(b) The second case is the reverse of the first. The extreme individuals shift their positions considerably, while the middle individuals are more nearly constant. The average distribution then has many deviations of moderate size, and the ratio of  $\sigma$  to a.d. is small.<sup>2</sup> The use of a.d. then gives higher values for  $r$  than the use of  $\sigma$ .

<sup>1</sup> In two 'substitution tests,' I found  $p = 1.18$  in the first test and 1.19 in the second, while it was 1.33 in the average distribution. Here  $r$ , by the Pearson formula or by formula (4), was  $= +.42$ , but by formula (14) it was  $+ .11$ .

<sup>2</sup> For example, in one 'form-naming test,'  $p$  was 1.25, and in another it was 1.12; in the average standings, it was 1.12. Here  $r$ , from the a.d. formula, was  $+1.00$ ; but from the  $\sigma$  formula it was  $+ .79$ .



In all such cases of a difference of result, according as  $\sigma$  or a.d. is the basis of calculation, the question remains open which result is the truer. Working with  $\sigma$ , as well as working with the Pearson formula, weights the extreme deviations rather heavily, and since extreme deviations are, in mental tests, often accidental, the weight thus given them may easily be excessive.

When a.d. has been used as the basis, and suspicion arises that the result would have been different if  $\sigma$  had been used, the value of  $\sigma_s$  can be approximately found, and all the values dependent on  $\sigma_s$ , without the need of going back over the whole work. If  $p_a, p_b$ , etc., be the ratios in the single tests, and  $p_m$  the average of these ratios, and if  $p_s$  be the ratio in the average distribution, then equation (12) may be re-written as follows:

$$\text{a.d.}_{ps} = \frac{p_m}{p_s} \sigma_s; \quad \text{or,} \quad \sigma_s = \frac{p_s}{p_m} \text{a.d.}_{ps}. \quad (17)$$

Now the ratio of  $\sigma$  to a.d. can be got from the reduced measurements for each test by simply computing the  $\sigma$  of the reduced measurements (the a.d. of these measurements being 1, since it was the unit of reduction), and  $p_s$  can be found from the a.d. and  $\sigma$  of the  $s$ 's. In a similar way, equation (17) enables one, after conducting the work in terms of  $\sigma$ , to find also, approximately, the result which would have been obtained in terms of a.d.

In conclusion, it should be said that the formulæ developed in this paper, in so far as they depend on  $\sigma$ , are not to be regarded as approximations, for they are mathematically equivalent to the usual formulæ. The formulæ developed for use with a.d. are approximations, if the others are regarded as the norms; but where the results of the two general methods differ, it is by no means certain, *a priori*, which gives the truer result.

## KNOWING SELVES<sup>1</sup>

BY JOHN E. BOODIN

*University of Kansas*

### I

In trying to know the self, we must recognize in the first place that our concern must be with the finite self and its processes. We cannot even conjecture a mind different from ours. Such a mind must turn out in the last analysis to be an abstraction from our own experience. The idealistic absolute is merely our own ideal of a completed knowledge, not a different mind.

In the second place, the method pursued must be naturalistic. We must strive to know a self as we try to define a chemical element—through its conduct, not through *a priori* considerations. We would not say that the self is its behavior, any more than we would say that a chemical element is its behavior. It is not only the way it behaves, but the way it *can* behave in all possible situations. The self is what it must be taken as in its behavior, by itself and by others, in various contexts, physical and social, especially the latter. It is not something over and above the properties as known in situations; the essence appears completely, given the proper conditions. There is no substance except energy. The self can be as truly known as a chemical element in the tests of various situations. It has its breaking point in the stresses and strains of experience as surely as cast iron; its melting point as surely as gold; its freezing point as surely as water; its explosion point as surely as dynamite; its point of confluence with other selves as surely as wine mixes with water; it separates from other selves as surely as oil refuses

<sup>1</sup>The doctrine of "pragmatic realism" has been defined in its general outlines in "Truth and Reality," Macmillan, 1911. Two other concrete studies in this series are "Knowing Things," *Phil. Rev.*, July, 1911, and "Do Things Exist?" *Jour. Phil., Psych. and Sci. Meth.*, Jan. 4, 1912.

to mix with alcohol. Habits, motives, characters are but expectancies of varying complexity, which we can have as regards the self with reference to definite situations. Of these situations, the social situations are by far the most important—the only ones in fact which would make self-consciousness in the first instance possible. But secondarily at least physical situations, too, count. In them we learn our strength and courage and many other properties. The self in any case is what we must take it as being in conduct. It has spontaneity, if we must acknowledge spontaneity. It is a mechanism, just in so far as we can treat it that way. We must learn to take the mind *as known*, and not as the epiphenomenon of material processes on the one hand or of a transcendent substance on the other. We must start with facts, not with dogmas. Its properties indeed are different from those of material things. It has no mass or weight. But neither has electricity. Its properties differ in different situations. But so do those of any physical thing. The visual properties of the diamond don't cut glass. While difficult sometimes to calculate, owing to lack of organization or owing to complexity of motives, still its conduct is largely predictable. Human institutions of credit and confidence are built on such predictability. Taken in the average such predictability becomes well nigh absolute. Where the self differs radically from a physical thing is in the fact that consciousness is superadded to its activities and so gives them meaning and value for the self. But while this adds subjective significance, it does not prevent us from taking account of the properties of the self, past and present. And the property to have mercy is just as much of a property as the solidity of steel.

Like radium, mind is not as yet known to exist in an isolated state. We know mind, for certain at least, only in connection with physiological processes though we may hope for more corroborative evidence of the existence of mind after death. But while mind exists in connection with physiological processes, we know it nevertheless as pure; and we know it better than we know anything else. When



we take account of our own meaning or try to understand another living mind or try to get the significance of a poem, in either case, nerves don't get mixed up with ideas, any more than the letters on the page get confused with the meaning we try to decipher. We know mind as it is. Whether we know its existence apart from certain physiological conditions or not, itself we know as clear and distinct, a fact of its own kind, with its definite internal as well as external relations.

## II

This is as true in knowing other minds as in knowing our own. The knowledge of other selves has been confused by two theories. One is the theory of analogy, viz., that we know other selves only by analogical inference, based upon the similarity of other bodies and their behavior with our own, while it is only our own mind that we know immediately. This theory confuses the problem of causality, with the problem of knowledge. It is true that our minds must make differences to our own bodies and their physical environment before their behavior can be overt to others and *vice versa*. But it is not true ordinarily that in knowing we argue back from bodily structure to mind. Man had composed great epics, laws and religions, built all the fundamental social institutions, before he knew there was such a thing as a nervous system. And even now the knowledge of the relation of mind and body is decidedly problematic and not to be compared with our knowledge of the mind's own relations, as we know it in logic, psychology and ethics. To be sure we sometimes start from structure in dealing with lower animal minds, but this is just the beginning of hypothesis, not real inference as to the mind's own nature. This must be understood through conduct—its intelligence and docility, quite independent of the presence of a nervous system. It certainly seems absurd to suppose that men should first study the connection of mental states and bodily expression in themselves and then read a mind back of the expression and structure of others—and that before they know anything about the connection of mind and body in themselves or

have even distinguished mind from body. It seems pretty clear that they start the other way; that they first learn to connect mind and conduct in others, before they become aware of the relation of mind and conduct in themselves. They learn to associate emotions and attitudes with expression in others before they are conscious of expression in themselves.

The other theory is the mystical theory. It argues for the immediacy of the knowledge of other minds without reference to expression. We immediately acknowledge other selves and that is all. Such acknowledgment is based upon no inference, implicit or explicit. It permits of no genetic analysis. Now this theory is certainly nearer true than the previous, from which it is a reaction. The knowledge of other selves may be regarded as immediate as that of our own. We know ourselves, as we know others, through the situations upon which we react. But this is quite different from holding that we have a mystic knowledge of ourselves in the abstract or of others in the abstract. In the abstract our significance equals zero. The knowledge of other selves is neither matter of analogical inference nor mystical appreciation but the homely way of reading conduct. And as social adjustment is a centrifugal process, it is natural that we should have formulated our own significance in terms of social situations—of social approval and disapproval, before we began to formulate the relation of social situations to our own ideals. The learning process is at first a purely objective process. A boy friend of three was confronted with a small misdemeanor. He recognized by the situation that it was a wrongness. He steadily maintained that he had not done it. His father sternly and sadly said, "Bobby, are you telling me a lie?" He was finally brought round to the right point of view with his mother's assistance and owned his act, with the solemn impression that the serious thing about it was telling a lie. The next day he astonished his parents by adding to an answer which he made "and it is not a lie." Through one tragic social context he had learned the significance not only of a word but of a social relation. It is

safe to say that he did not compare the parent's bodily expression with his own.

In the progress of experience, language as an artificial expression of mind, with its complex network of relations, largely takes the place of concrete situations for knowing minds. And our knowing our own mind, past and present, as well as knowing other minds, becomes the immediate recognition of the meaning of the language situations, until in the technical disciplines concrete imagery very largely drops out in our reading of meanings. The matrix of language, with its artificial equivalents for things and relations, becomes the social correlate of our communication and understanding of minds, not brain cells and association fibers. And logic, geometry and ethics as sciences of social mind relations reached a high perfection as sciences before neural physiology was born. In social communication, what we are immediately concerned with is words, conduct—not brain states. In talking with an individual, as in reading a book, we are concerned not with causes and effects—the producing of the spoken or written symbols and their reaching our, or the other party's sensorium. We are concerned with the interpretation of the symbols. The words are immediately associated with certain meanings; and our attention is fixed on the meanings, not on the instruments. As the ivy clings to its material framework which supports it, so do our meanings in every joint cling to language, only the meanings make their own framework as the nautilus builds its chambers.

While it is true that in understanding other selves we are dealing with the social matrix of language and meanings, still this does not prove that brain processes, nerves, vocal chords, air-waves and ears or eyes do not mediate causally between selves in communicating with each other. The teleological explanation and immediate acknowledgment of meaning by meaning would not be possible, in our sense world at least, if the communicating minds were not part of the causal nexus of the intervening world.



## III

A great deal of mystery has been thrown about the dual nature of the self by traditional psychological theory. It is supposed that there is an absolute and constant distinction between the *knower* and the *known* or, to use James's phraseology, the *I* and the *me*. This is true not only of the old rationalistic psychology with its metaphysical soul, but it is true of recent treatments. Says Wundt: "Every experience contains two inseparable factors—objects of experience and the experiencing subject."<sup>1</sup> And Ebbinghaus: "Wherever thoughts and sensations are experienced, this subjective bearer to which they adhere, also becomes directly conscious in them and through them, in the same way as they themselves."<sup>2</sup> And even James: "It is obvious that if things are to be thought in relation, they must be thought together or in one something, be that something ego, psychosis, state of consciousness, or whatever you please."<sup>3</sup> This something to be sure is a "a spiritual something." Still its absolute distinction from the known content is implied. When I try to make clear to myself what this simple bearer is which is constant in all the states and logically distinct from them, it seems to be nothing else than the abstract fact of consciousness itself. This certainly is constant and simple and accompanies all our conscious states. It is also separable from them, as mind stuff need not be always conscious.

If what is meant is that all experience involves the subject-object relation or is representative, certainly some doubt may be thrown from the side of the facts. When we think, we of course always presuppose the subject-object relation, but is this true also of the simpler perceptual stage of experience? Could a creature depending upon impressions and upon learning by habit, without any images, say I? This does not seem likely, because there is no conscious context, which assimilates. In all experience too, there must be a beginning, a bare "awareness of" without any

<sup>1</sup> Wundt, *Definition der Psychologie*, Philosophische Studien, 1895.

<sup>2</sup> Ebbinghaus, *Grundzüge*, Vol. 1, p. 10.

<sup>3</sup> *Principles of Psychology*, Vol. 2, page 277.

"knowledge about," *i. e.*, without any associative context to react, where our experience *is* the light or the pain rather than *has* it. I do not see how such experience could have the "two inseparable factors, objects of experience and experiencing subject." In my own experience in waking up gradually after having been struck by lightning and snowed under in a storm on Gray's Peak, I could remember afterwards when I was a mass of pain and discomfort with no associations suggested in the way of danger or death. I could remember having seen the form of a man moving down the mountain side. But it was not until some time afterward that the perceptual picture suggested man and a futile cry for help and not till long afterwards that the perception suggested my companion and that the scene itself came back to me. There was certainly a period there of pure perception, while the associative context was as paralyzed as my bodily movements. I believe, in other words, that there is a simpler state of consciousness than the *I* and *me* relation—the state of bare awareness, which, of course, is not broken up except by a later more complex consciousness, which reflects upon it. Leaving aside, however, the question of the universality of the subject-object relation, what does it mean when we do have it? And is it such a mystery?

The mystery and the absoluteness and the constancy of the subject-object relations seems to disappear when we bring a little psychological analysis to bear. Abstracting from consciousness as bare awareness, we must make clear to ourselves what we mean by the subject-object relation in the concrete; and then we shall see that it is an associative context assimilating a selected content—the datum. The quality of *my*ness is a function of the datum-being-selected by this individual interest. In other words, to say this is *my* object of consciousness and to say I am interested in this object are two different ways of saying the same thing. The *I* or subject in this relation is the active associative context, which we call interest, solicited by or striving to find its object, the *me*. In order to get rid of ages of false association we may call this context the *referent* and the datum, which is

selected, the *referatum*. Now my contention is that there is no absolute or constant relation between the selective context, the *referent*, and the selected object, the *referatum*. On the contrary, the distinction is relative to point of view and relative to time. In the first place, it is relative to point of view. This may be true within the same physical individual, as in the case of the divided self. In deliberation, the point of view shifts while the systems seem exclusive and constant. Now one system is tried out with reference to its antagonistic systems. And again the activity shifts to another system with its scale of values. But the struggle is precisely between coexisting and conflicting points of view. Here of course there is some common and constant group of tendencies which figures in the various systems and accounts for the shifting of attention. It is this common group of more or less implicit tendencies which gives rise to the feeling of outside push as regards the process of deliberation. The systems in intense moral struggle may coexist antagonistically for some time—each with a strong individual consciousness, and each struggling for the place of mastery, now one, now the other occupying the focal place—the lower taking the higher captive, the higher in turn summoning its energies against the lower, each very much alive and struggling for existence. In insanity we have cases of actual disruption of the various systems, when a man feels himself to be not one but many—thoroughly bewildered in the shifting and many-headed focus, as to which is I rather than the other. In every case of social communication where one system of meanings strives to understand another, the relation of *referent* and *referatum* is reciprocal and a matter of point of view.

Not only is the relation of I and me relative as between *coexisting* systems, internal or social, with their respective points of view. But the *same* system may now be *referent* and now *referatum* in the one personal history. In recalling a forgotten name, we use the meaning to find the name. The system of associations with its leading is the *referent*, the name the *referatum*. But having gotten the name, we



reverse the process and use the name with its larger context to fix the meaning. "It is my purpose to open a refractory door. This system of tendencies, the *referent*, hunts about for means, the *referatum*. But in trying to solve the door situation, the purpose becomes aware of its own vagueness and limitations. It thus reverses itself in a measure; the door-consciousness with its associations defines the purpose to open and both are taken up in the larger context which was implicit in the procedure—getting what I wanted in the room, etc. The relation of I and me then is not a constant or absolute relation. There is no more mystery about the I than about the me. They continually shift places and *referent* becomes *referatum* and *vice versa*. They are both functions of a more or less definite system of tendencies which strives to realize itself and which we may call the self in the inclusive sense.

If this theory is true it should follow as a corollary, that self-consciousness, *i. e.*, consciousness of the I and me type, should be prominent in proportion to the activity of attention, being particularly obtrusive in the moments of embarrassment and frustration, while approaching the vanishing point with the fluency of the on-going of consciousness, when the felt unity radiates in all directions of the prevailing purpose. This seems actually carried out by the facts of experience. It is when the developing purpose is brought to halt, is balked for the time being, that the consciousness of meaning and datum, *referent* and *referatum*, becomes painfully strong. On the other hand when the flow is uninterrupted, when the purpose is absorbed in the transitions from phase to phase, whether the fascination be intellectual, practical or esthetic, the dualism of I and me approaches its vanishing point until lost in the mystic trance—the passive, coalescent state of attention.

Hume in speaking of the self as a "bundle of perceptions" fails to take account of the active dual character of the self. The self, whenever we are awake, is an *active* bundle of more or less systematized tendencies striving to appropriate or adapt itself to any external context, the datum. It is the latter context that has the stubborn perceptual character.

The self is not a bundle of perceptions. It is a bundle of tendencies, leadings, purposive striving. The perceptions, whether internal or external, are the facts taken account of—the me. You can't have an outside without an inside; and Hume<sup>1</sup> made the self all outside.

#### IV

Again, when we come to the identity of the self, we must hold that the self, like physical things, is just as constant as we can take it—as constant as its activities and contents; as its ability to satisfy social expectancies. What is the use of assuming, beside this constancy of the stream of consciousness, a substance to make the process constant? Such a substance is obviously an afterthought, an hypostasis of the fact of constancy itself. It makes the processes neither more nor less constant than they actually are in the flow of experience. As the organic sensations constitute a constant background in the changes of mental life, they play an important part in our consciousness of identity. They furnish largely the warmth and tone of personality; and disorganization of these sensations produces serious disturbances in our sense of self; yet to furnish the meaning of identity, there must also be certain constant tendencies, which furnish the leading or active thread of experience in the panoramic and shifting scenes of feeling, perceptions and ideas.

Kant is quite right that the consciousness of succession is a different fact from successive states of consciousness. But it does not follow that the consciousness of succession requires any transcendental unity outside of the stream of experience. The consciousness of succession means that a relatively permanent system of tendency, in order to realize its will, must take account of the coming and going of contents—must emphasize the constant as over against the fleeting in order to establish definite expectancies. What furnishes the pragmatic substance is precisely this core of permanent tendency and associations in the midst of the flux. What is the use of duplicating this identity by assuming an-

<sup>1</sup> *Treatise on Human Nature*, Vol. I., Part IV., 6.

other identity to account for it and so on *ad infinitum*? Why not take the constancy of the processes, so far as such constancy must be acknowledged, at its face value?

Constancy and change are both facts that the will must acknowledge and meet in the process of experience. There is the relatively permanent will, the invariable associations; and there is, on the other hand, the shifting of contents and values, the new experiences, the unforeseen obstacles, the pleasant surprises. Why not take experience as it is? But this is not human nature. Our tendency is to emphasize some aspect of the whole and neglect the rest. For instance, you must admit that there is constancy in experience. If that is the case, one argues, there must be absolute constancy. In and through the states, there must be an eternal self, a transcendental substance, which remains identical in all the states. And the changes themselves are merely accidents of this eternal substance or character. And, on the other hand, there is change. Our mental facts come and go. Very well, then there must be absolute change. There can be no constancy if there is change. Each gross moment is really divisible into infinitesimal changes. And everything must necessarily flow through these infinitesimal transitions. There is only appearance of qualities or contents, but really there is this absolute flux. Does the attention vary? Then the contents attended to must vary also. Do our ideas vary? Then our will attitudes must vary too. Such is the reasoning the human mind from age to age has employed; and new editions are appearing all the while. But what we must not forget is that the conception of an eternal self, on the one hand, or of the calculus of flux, on the other, are merely tools with which we work. Their authority in the end can never rise above the facts from which we have derived them. The contents and tendencies may overlap our ideal divisions. All we can say is that in the history of the self there is change and growth and novelty, but there is also some constancy. Else we would not even be talking about flux. There would be no memory or expectancy. We must learn to recognize constancy in so far as there is constancy and flux in so far as there is flux.



Just why there should be such a mystery about the continuous occurrence of a relatively stable context, taking account of a series of successive feelings or perceptions, it is hard to see. But such has been the feeling of others beside Mill: "Accepting the paradox that something which *ex hypothesi* is but a series of feelings, can be aware of itself as a series. . . . I think by far the wisest thing we can do is to accept the inexplicable fact, without any theory how it takes place; and when we are obliged to speak of it in terms which assume a theory to use them with a reservation as to their meaning."<sup>1</sup> Yes, perhaps. But would it not be wiser still not to invent such an absurd paradox? The series of feelings does not as a series know itself, but is known as such by a context of interest which is for the purpose stable. And, again, while our ability to control the series of feelings, so as to keep them in the focus of attention may be circumscribed by a few seconds, it is not necessary to suppose that the interest in controlling them is so limited. The context of interest may be lifelong. To measure the real permanency of the self by the flicker of attention, whether we have recourse to the infinitesimal calculus or finite fractions of seconds is equally mistaken. The real specious present is just as long as the associative interest, determining the series of events, whether we attend to such interest or not. The time-span of the self and the time-span of attention should not be confused. As a matter of fact while attention flickers we can bring back again and again the contents and will attitudes.

The past is not made by the consciousness of it any more than the present. You might just as well say that the geological strata originate with the consciousness of them. The past has its own context and its own content as much as the present. The past comes to have significance for the present moment, when it is attended to; but its own meaning does not originate then. If it did, there would be no possibility of knowing the past, because there would be no past to know. The context of the past must somehow persist and be acknowledged by the present, if we are to have a past. Else

<sup>1</sup> J. S. Mill, *Examinations of Sir Wm. Hamilton*, 4th ed., 247 ff.

there could be no memory. Take the simplest case of recognition, ideal or perceptual. Part of the past content must figure as a content in the present context. This content in definite recognition reinstates its own past setting, be it the sensory context of the perceptual object or the ideational setting, which is acknowledged by the present context, including the dating. There may, of course, be all degrees of reinstatement, and so of vagueness of recognition, but in any recognition a past content must figure as the identical content in the present context together with its own tendencies. In this unraveling of the past context, there is also the more or less vague intuition of pastness, due to the growth series of which the contents are a part. Sometimes an identical content plus this feeling of pastness is all that figures in the present context, and we have the confusion of feeling that we have been in the same situation before, when we know we cannot have been there.

In purposive realization, we have a similar illustration of constancy with reference to the future. If ideas could not persist, but were new every moment and in each infinitesimal fraction of a moment, there could be no such thing as the realization of an end. Experience would be one immediate slough without direction. The facts of experience, however, show that we can keep a constant nuclear aim, however much the context of our meaning may grow in extent and definiteness in the process. There remains an identical content or constellation of content through it all. And so ideal realization is possible.

It is true that we depend very much upon symbols in retaining the past and in fixing the present and the future. Knowing our own meaning, past and present, as knowing those of others, is largely an interpretation of language. But language after all is only the symbol of the contents of experience. Language could not convey the same meaning, unless we owned the meaning. We can recall blue sky when we perceive the words, because we have the actual meaning blue sky, however fragmentary its concrete content. And failing this, as in an unknown tongue, we would simply have

words staring us in the face, conveying nothing. Language, moreover, is discrete and stereotyped and fails to give an equivalent for the quivering transitions that persist indefinitely within the systematic meaning. It is not fair to substitute the tool, however important, for the living reality.

But, we shall be told, the real persistence is not of the contents themselves but of brain-processes. We have already pointed out the absurdity of supposing that our mental contents are converted into atoms and molecules, when we are not attending to them or still more that they should disappear into nothingness to be magically recreated. All that the brain cells can do is what the phonograph or the camera or the written page does for our senses—furnish a record of experience. But just as the mind must furnish the real content for the written page and the other sense records, so it must furnish the content for the brain record. The brain record no more makes the content than the words on the page. And if the brain record *means* a constant record it must be because the content is recognized as the same. As a matter of fact, owing to the uncertainty of the brain record, we substitute largely the ink record which is both more reliable and socially more available. Whenever, then, we have the meaning of identity and not bare physical identity, there must be the identical content. That is the real currency of which the records are the symbols. That we are immediately aware of the brain record and only through the senses conscious of the ink record does not alter a particle the significance of the records. When we read the ink records, we do not read the retinal fibers or light rays; we interpret them as symbols of content, just as much as though they were written on our brain by the law of habit.

If this statement of the facts is true, then if the contents fade or become dissociated, we in the same degree fail to recognize identity in ourselves or others. This is actually true. The mental energies have their own laws of spreading and becoming ineffective as shown by the researches of Ebbinghaus. Sometimes our associative contexts become dissociated; and, when they do, no transcendental ego breaches



the gap. Memory and recognition operate only when there is constancy within the *referent* context. The other contexts, the *referata*, can suggest neither sameness nor novelty, unless there is such constancy in the subject. How far the inefficiency is due to records can be ascertained by the fact that when an objective record is present, visual or oral, the contents are reinstated even when the brain record is ineffective. This fails when there is real dissociation of the context of meaning.

We must, finally, remember that the constancy of the individual meaning is determined not merely by the individual himself, but by his social context of relations, which reacts upon his own consciousness and with reference to which constancy and flux alike become practically significant. This social context of judgment with its records must determine how far the individual feeling of identity can be trusted. The individual meaning sometimes judges itself to be constant, when the social verdict is otherwise. And identity has significance primarily as a social category.

## V

The unity of the self is a question distinct from the subject-object relation. Whether the self functions as whole or part, there is, whenever it is awake, the focal and marginal field, the active context and the selected content. This is true even where there is complete dissociation of associative systems. Leone II., when awake, has just as much the I-me character as Leone I., however distinct they may be as regards associations, temperament and character. To what extent, then, the experiences of one organism hang together must be treated as a problem by itself.

The self has as much or as little unity as we must recognize. We must take unity precisely as we are accustomed to take difference—at its face value. And here again no *a priori* assumptions will either increase or diminish the unity which we find in the actual processes. As a matter of fact a transcendental ego or soul, or any other additional entity, spiritual or material, simply becomes one fact more to be related to

the rest; and if we dogmatically deny unity to the facts themselves and treat them as purely unique and disparate, no external linkage will serve as cement to bind the hypothetical differences together, not even in an infinite series. The processes cannot be unified by being "in something" external to themselves, material or spiritual. They must possess their own linkage.

The dogma that each state of consciousness is unique and indecomposable is as indefensible as that of a transcendental knower relating disparate facts. There could be no judgments in an experience where the facts are so intimately blended that the experience could only be taken as a whole. All our thinking depends upon our ability to analyze out identical qualities or relations and to pass from one context to another on the basis of such abstraction, dealing with the contexts only so far as the identical predicate pertains. As a matter of fact actual experience shows that we can take objects and qualities now in one context, now in another, without altering the object by thus subjectively taking it. The content yellow when transferred from the marginal to the focal field of *attention* is not altered in quality, whatever may be the difference of moving it from the marginal to the focal field of *vision*. We can know the past, we have seen, and we can prepare for the future, because we can take the contents and their contexts over again without altering their own meaning or reality in so taking them, however much their significance for the cognitive context may be altered. The dogma of the uniqueness of each state of consciousness as such contradicts all our procedure and would make not only the science of psychology but any science impossible, for all science presupposes the possibility of taking facts over again in experience.

There are doubtless unities in experience which fulfil a unique purpose and which as individual unities cannot be exchanged for other unities. But why, therefore, give our entire experience this character? And even when a whole has uniqueness, it does not follow that the elements cannot be analyzed and taken over again indefinitely. Unique as

the Angelus is as a painting, the elements of color perspective and form can be analyzed out and can figure in any number of contexts. The composition is not unique in its elements, but in the *will*, which they express through their correlation and which furnishes a specific satisfaction to the appreciating subject.

Absolute uniqueness, like the absolute ego, is a dogma unsupported by facts. Why make knowledge impossible by assumptions? If experience came merely by unique throbs, prediction and knowledge would be impossible. If it consisted of wholly diverse contents, knowledge would likewise be impossible. But to some extent we can have prediction, we can pass from fact to fact by means of identities in the stream of processes. Let us take as distinct what experience makes distinct and what experience joins together let no man's assumptions put asunder.

If we insist on abstracting from the processes of experience altogether—from its active context and its selected datum—in order to discover something which is not content at all but which is super-added to the activity of the self, we get not a transcendental ego or any other common entity outside of the process, we get the fact of consciousness in the abstract which accompanies all our apperception. \*This is not a spectator. The spectator of experience must be the apperceptive context. It is merely the condition of awareness at all. It is quite colorless. It is responsible for neither diversity nor unity. It holds together as little as it separates. It simply makes the facts apparent. It is not necessary to give consciousness an individual existence for each knower as is done with Purusha in the Sankyah system. The individuality belongs to the processes themselves, in their phylogenetic and ontogenetic history. Consciousness taken as an abstraction is homogeneous.

Sometimes we must recognize the self as partial. It may be a case of impulsive control where the total context of tendency fails to express itself. We then speak of ourselves in retrospect as not being ourselves. It may be a dissociation of memory, which makes us fail to connect with the past



and so carry out our obligations and satisfy expectations. It may be organic disorder which makes one part of the content, previously linked with the self-content seem foreign altogether, with altogether strange properties. It may be a case of more profound dissociations, where disremented systems of association, each with its own characteristic tendencies simultaneously or alternately, struggle for the mastery. In all of these cases the thus dissociated contents may come to figure again, after restored equilibrium, in a total context with its characteristic consciousness of identity. But, in any case, we must accept the actual association or dissociation as it is.

Even when the facts hang together, they do not hang together in the same way. There are different grades of unity. This has not always been recognized. Because some facts hang together systematically as parts of a purpose, it has been argued that the self is fundamentally a thinking or rational self and that all mental activity is implicitly or explicitly of the judgment type. The realization of an ideal self, then, becomes a case, not of empirically selecting and composing a unity in obedience to a purpose, but of becoming conscious of an eternal self already constituted and implicit in our simplest mental acts.

Again, because some facts apparently hang together only externally, in space and time, in our attention moment with no seeming internal bond, but simply interpenetrate or stay side by side in the field of interest, it has been argued that habit or contiguity is the only real linkage of our mental facts, with similarity given perhaps a more or less vague secondary place. Or, perhaps, the opposite: since similarity of content is sometimes found to be the seeming linkage, contiguity is made a case of similarity, if the two are not simply held out as irreducible laws. This external linkage, moreover, has generally been credited, not to the side of mental processes, but to the account of brain processes. Our ignorance of brain dynamics and the paradox of cementing feelings and ideas by means of atoms and molecules and their habits has not discomfited physiological psychology.

Now here again we must take connections at their face value. In part, evidently, the facts of mind hang together as members of a system. They cohere and are controlled by an identical purpose—utilitarian, logical, esthetic, ethical or religious. It may be the idea of wealth; it may be the pursuit and creation of beauty; it may be the love for truth; it may be the passion for righteousness; it may be the imitation of Christ. The ideal self is no doubt the self unified within a comprehensive purpose, where all claims are adjusted, all facts seen in systematic relation. But for us finites that is largely an aim and only in part fact.

The linkage sometimes is the consciousness of a common quality or relation by means of which the otherwise seemingly heterogeneous facts or contexts hang together. Thus consciousness of a common quality may bring together processes which have never been experienced together and so is quite a distinct and elementary fact. In every case of recognition, however rudimentary, this consciousness of partial identity of traits of one individual or context with another is present. This, in the case of the judgment of analogy—the building of expectancies upon identical traits—forms the transition to the reading of facts by internal connections rather than adventitious. That one individual or setting is merely like another is an adventitious relation. It is when the identity links them in a system of prediction, that we have science.

The loosest or most adventitious bond is that of habit, the contiguous interest in facts merely happening together in space or time. The overlapping part of one context will tend to reinstate its other context or one of its other contexts in accordance with the strength of the habit so established and the dominant set at the time. The set of active tendency acts like a switch system, making some habits effective and others ineffective for the specific purpose.

Whether the linkage be through a systematic purpose or through similars or through habit, in any case the linkage is due to the consciousness of identity in the variety of facts and contexts. There must be at least the bond of unitary interest, whatever may be the own linkage of the facts them-

selves—the identity of the context which takes account of the facts, fleeting and heterogeneous as these facts may be. While a great deal of our mental life is held together by such contiguous interest, we must not forget that it is precisely in the mental world that we can follow the transitions from fact to fact and have the immediate consciousness of their string of identity, *i. e.*, wherever we have purposive unity. Outside of what appears to be mind we must be satisfied to piece out the transitions conceptually and inferentially. We cannot follow immediately the transitions of the facts. It is a mistake to ignore these internal transitions of the mind and to strive, as modern psychology has done, to reduce the relations of mind to the adventitious and external kind. Purposive unity must be recognized together with passive association by similars and by contiguity as an actual type, as well as an ultimate ideal, of mental connection.

## VI

What becomes of activity and freedom, if we once admit that the stream of tendency is the agent? No theory can unmake the facts and such activity or freedom as there is still remains. It is hard to see of what use a transcendental knower or any spiritual something could be in accounting for activity or any other function of the self, as it must necessarily be the same whether the self is active or passive. Activity, as contrasted with passive and non-voluntary states, would still have to be stated in terms of the concrete processes, though somehow it is hard to rid ourselves of the feeling that the idea of an extra entity, added to the facts, affords an additional guaranty of identity, unity and freedom.

Activity has sometimes been identified with the unpredictable and novel. The world of psychic reality, it is rightly pointed out, is not exhausted in our concepts. It is not all included in our present logical context of meaning. One characteristic fact about the stream of consciousness is its ever novel situations and novel attitudes. You cannot but partially predict the future. But however true this may be, as stating our finite experience, and however much we may



recognize the truly temporal aspect of life, it does not seem possible to define activity in such terms. For change and novelty exist apart from our activity. They confront us whether we are active or passive, awake or asleep. They may come to thwart our purposes as well as come as the fruits of our efforts. I do not see, therefore, how we can define activity or freedom in terms of novelty.

Moreover, activity and freedom must mean the realization of an aim. And we cannot aim at the unpredictable. We can only wait for it and let it happen as it may. Activity on the contrary means the control of events—ideas, feelings, perceptions, impulses—by an idea which remains constant. It is just this conscious leading that we mean by the self, in our awake moments. That the novel and unforeseen happen is incidental to the activity and may be forced upon us quite independently of our being active; though a general readiness even here is the thing, in turning the novel to our advantage when it comes. Activity means that the consequences follow from our intention, not contrary to our intention.

Activity, therefore, is the very opposite of chance, which means the unpredictable and uncontrollable. Fluent our world must be to some extent to have expectancy, to look to the future; and with the fluency there may be novelty. Perhaps there always is. But freedom relates to making real the ideal content, to regulate the flow in accordance with the dominating purpose. Only as the idea can recognize the results as its own fulfilment, however much more definite and concrete, can we have freedom. Whether the flow in such a case is determined altogether by 'considerations,' or whether it is to some extent independently variable, does not concern the question of freedom, since we are only free in so far as the flow *is* controlled by the purpose, *is* organized into some ideal scheme of life. The novelty, as we finites can take account of it, is necessarily an afterthought, a gift of the process, and by hypothesis it is not anything we can theorize about. It is a character of the concrete flow, as contrasted with the expectant and guiding idea. Whether it is absolute novelty

or due to the peculiar limitations of our experience it is difficult to know. Some of it is evidently due to the limitations of our finite consciousness, where we often discover that what is novel to us as individuals or even as an age is already part of the content of historic humanity. The novelty of the child, to whom all is novel, is merely its taking over of the content of the race, barring its own specific organization. But while novelty abounds in the infant's life, there is no freedom. Conduct is free only when it is a consequence of a systematic purpose. We don't call a man free in so far as he must say of the outcome of his conduct: "That is not what I meant" or "I had not thought of that."

## VII

Nor does this theory destroy the value and worth of conduct in so far as it possesses those characters. Any abstract entity added to the processes would as little account for the presence as the absence of value in any specific process. Value is a function of activity. Objects have value when they satisfy some tendency of the self, in its various stages of complexity and equilibrium. As every satisfaction of the will is a value, we must distinguish between value and worth or subjective value and objective value. Whether an activity has worth or not does not depend upon its individual satisfaction, but upon its agreement with a standard which the will must acknowledge. It may be the standard of social agreement; and this at any rate is enforced upon the individual will, whether accepted by it or not. But as this too is variable, our finite activity, in order to have worth, must refer to a standard which the social will, too, must accept in its racial development. The meaning of this we can only catch gradually, and every such advance in meaning must come from individual insight.

As the identity of activity with an objective standard is worth, so, if there is an ultimate and eternal standard, agreement with this standard means immortality. As society conserves the unities which harmonize with its standards, and makes the poem, institution or individual that expresses its

will immortal, in so far as in it lies, so, if there is an ultimate standard and a will to enforce it, this will must intend the immortality of that which realizes the standard, be the unity personal or impersonal. The worthless unities could not, in such a world, survive as individual unities, they could only survive as contents or tendencies to be used as raw material for more comprehensive unities, as the button moulder in Ibsen's *Peer Gynt* melts up the sham individuals in his ladle.



## DISCUSSION

### PROFESSOR TITCHENER'S THEORY OF MEMORY AND IMAGINATION<sup>1</sup>

The theory of memory and imagination of which this paper proposes a partial discussion cannot be understood without a clear comprehension of the terms in which the author carries on his exposition. It is to these terms, therefore, that we shall first give our attention.

The division of the *Text-book of Psychology* that we are concerned with in this review deals with the recognitive, the memory, and the imaginative consciousnesses. Titchener's exposition of each of these processes falls under the three following rubrics: the "focal process," the "essential factor," and the "typical forms." The term "focal process" indicates the character of the process that is central in a given case. In memory and imagination this is ideational, while in recognition it is perceptual. The term "essential factor" means "what is characteristic of" the consciousness under consideration. By "typical forms" or "types of consciousness" is meant the kinds, or classes, or species of the consciousness being considered.

A partial presentation of Titchener's view of these three forms of consciousness will be the following:

The Consciousness	Focal Process	Essential Factor	Typical Forms
Recognitive consciousness . . . . .	Perception	Feeling of familiarity	Definite
Lack of the recognitive consciousness . . . . .	Perception	Feeling of strangeness	Indefinite
Memory consciousness . . . . .	Image	Feeling of familiarity	Remembrance Recollection
Imaginative consciousness . . . . .	Image	Feeling of strangeness	Reproductive Constructive

If, now, we consider what Titchener has to say about each of the "typical forms" of the memory and imaginative consciousness, we are led to another set of terms. Two passages are immediately important. Remembrance and recollection, the "typical forms" of the memory consciousness, the author says, are "discursive; that is, are characterized by wandering of attention, shift of imagery,

<sup>1</sup> *A Text-book of Psychology*, pp. 396-427.

variable play of association." Reproductive and constructive imagination, the "typical forms" of the imaginative consciousness, are "integrative rather than discursive; the sphere of attention is limited, the play of association regulated." An interesting, and for the view under consideration an important, question is suggested by this way of stating this difference. Are we to understand that "discursive" and "integrative" are additional "essential factors," or are they something distinct? By an "essential factor," as we saw, was meant what is "characteristic of" a process, and in the passages just quoted, "discursive" and "integrative" are interpreted in terms of what these processes are "characterized by." Are these two phrases, "characteristic of" and "characterized by," synonymous? If so, the "feeling of familiarity" and the "feeling of strangeness" as the essential factors of the memory and imagination consciousness, respectively, need to be supplemented by the other pair of terms to make the former account complete. If this is not the meaning, have we here a shifting of the ground, that is, a change in the point of view from which description is taking place? The meaning assigned to the terms "discursive" and "integrative" suggests that this is the case. For by these terms we are made acquainted not with what the memory and imaginative consciousness are, but with what they become. They are interpreted in terms of attention and the play of association. And, in harmony with this interpretation, we are told in the immediate context that each form "shades off" into other related though distinct processes, and later that these processes, which I shall refer to as "genetic forms," constitute what the author calls a "psychological circle." Unless "discursive" and "integrative" mean something genetic, it is difficult, if not impossible, to interpret the context in which the distinction occurs. If this is the author's meaning, and if we may use the sign  $\rightarrow$  to indicate the phrase "shades off," we may represent the doctrine in the following schema:

	Genetic Characteristic	Typical Forms	Genetic Forms
The Consciousness			
Memory consciousness	Discursive	Remembrance $\rightarrow$	Day dreaming $\rightarrow$ Imagination
		Recollection $\rightarrow$	Inquiry $\rightarrow$ Thought
Imaginative consciousness	Integrative	Reproductive	
		Constructive $\rightarrow$	Thought

So much for the essential features of the doctrine. In what follows we shall be concerned with the meaning of what Titchener says. This will necessitate a constant reference to the many details

that necessarily have been omitted from the schemas above, and will go some way toward mitigating whatever injustice has been done the view by its schematic representation. But in the main we shall confine our remarks to the problems set by the two tabular views given above. And the first question is, what does Titchener mean by memory and imagination.

To start with the memory consciousness, we have here, according to the author, a process that is identical with the recognitive consciousness with the exception that the "focal process" is an idea and not a perception (p. 413). What, then, is the recognitive consciousness? It is a form of consciousness that is dependent upon the "secondary effects of (sensory) stimulation." These secondary effects "give us the key to the psychology of recognition" (p. 407). The secondary effects referred to are (1) organic reaction, and (2) feeling. Sensory stimuli, Titchener holds, set up both of these in addition to arousing sensations, and it is upon this fact that he bases the distinction between the "typical forms" of the recognitive consciousness (p. 409). If now we apply these statements, *mutatis mutandis*, to the memory consciousness, we may suppose that it also is dependent upon organic reactions and feelings, and that these are mediated by an idea. This, at any rate, is what it would be if, as the author says, "the sole difference is the presence of an idea in the one case and a perception in the other" (p. 413). But to take the passage immediately following the last quoted, "An idea is a memory if it is accompanied by the feeling of familiarity; and an idea is specifically remembered if it is placed and dated by the organic reaction and by associated ideas." Now, apart from the possibility, which seems to be granted, that we may have a memory which does not involve organic factors at all, we may ask why the statement has been given the hypothetical form. The reason is, according to Titchener, that "no image or idea is intrinsically a memory-image or memory-idea" (p. 413). The meaning, accordingly, is that when an image or idea is accompanied by a feeling of familiarity it is a memory, and when an image or idea is accompanied by organic reactions and associated ideas which give date and place to it, it is a specifically remembered image or idea.

One or two questions suggest themselves in this connection. The most concrete, if not the most obvious, inquiry relates to the method by which the organic and feeling factors that accompany the memory consciousness get aroused. If we subtract the organic and feeling elements from a memory consciousness there is left, presumably, an image which is not intrinsically a memory-image. As the author



says, "an image is made into a memory-image by the feeling of familiarity" (p. 419). If the image is not intrinsically a memory-image, how, psychologically, are we to regard this addition that is essential to the existence as a memory consciousness? In the case of the recognitive consciousness the matter is clear. For there, whatever arouses the sensation arouses at the same time the other elements that give the recognitive character to the experience in question. It would appear that it is different with the memory consciousness. Or may we say that whatever arouses the image arouses at the same time the other elements that impart to the experience its character as memory? If not, does the *Text-book* provide a different answer? It should be observed, to avoid misunderstanding, that our question is not met by what the author has to say on p. 414 f. where he is speaking of the pattern of consciousness in remembrance and recollection. The difficulty that the question is intended to call attention to arises, we think, from the author's identification of the memory consciousness with the recognitive consciousness with a single, which is said to be a sole, difference, and from his inability to meet the expectations aroused by this statement in the further study of the memory consciousness. In the detailed study, the memory consciousness seems to get separated by a greater distance from recognition than is compatible with the earlier account of their relations, and several differences, instead of the original one, seem to develop. What one notices here is a tendency to unite memory with a process—recognition—that has a sensory basis, and, then, to react gradually from, or to refine the position until one is left wondering whether these processes have anything to do with one another. This backward and forward movement, we may note by the way, is quite characteristic of the author's treatment of most of the topics with which we are concerned in this paper.

Another question, more general in its nature, and connected with the foregoing, relates to the standing of the memory consciousness itself. As we come away from the reading of Titchener's exposition we wonder whether there is any such thing as memory which is not either a remembrance or a recollection. This view is, of course, what we should expect from a psychological study. For psychology, there is no such thing as memory, there are only memories. And these memories are, in the terms of the *Text-book*, either remembrances or recollections. This view, however, does not accord with the exposition, for, as we have seen, memory differs from the specific memories by the absence of the organic factors that characterize

remembrance and recollection. Memory, moreover, is set off from its "typical forms," in a positive way, by the presence of the feeling of familiarity. Both positively and negatively, therefore, memory is distinguished as a separate process from the kinds of memories that one may have. We are not saying that this may not be; we are only wondering whether this is Titchener's view, or whether we are to take memory as a class term which is logically distinct from, but psychologically is identical with the items that are classified under it. We may remark by the way that the same line of comment applies to the author's use of the term imagination, and to the exposition of this topic we may now turn.

The main views of the author concerning imagination need not detain us long because the treatment here follows closely the lines laid down in the section on the memory consciousness, and because, as the author says, "we know very little indeed about the imaginative consciousness." Reference to the first table given above will show that an image or idea is a case of imagination when it is accompanied by the "feeling of strangeness," and we are told in other places (cf. pp. 417, 423) that with respect to the organic factors involved imagination is distinguished from memory by the fact that these are in this case empathic rather than kinæsthetic. Further, the author holds, although he does not state it explicitly, that in this case also no image or idea is intrinsically an image or idea of imagination, and he definitely affirms that "an image is, psychologically, made into an image of imagination by the feeling of strangeness" (p. 419). In this process, as also in the memory consciousness, it would seem that if we were to strip off the feeling and organic features we should recover a residuum which in both instances is the image, and if this is the fact, it is doubtless necessary to examine the doctrine of the image as a means of understanding the theory of memory and imagination that is now before us.

We have spoken in the foregoing as if the term image were univocal and in doing so we have merely followed the author in the passages referred to. In this paragraph we shall present, as far as possible in the words of the *Text-book*, the meaning to be assigned to this term. The fact that the important passages are widely separated has affected, if not the doctrine, the form in which the doctrine is presented. These circumstances under which we are compelled to approach this topic make it barely possible that we may at times have missed the essential meaning. However this may be, the important passages are on pp. 48, 197-198, 420-421. The first passage

occurs in connection with the question of the nature and number of the elementary mental processes. As this is a somewhat fundamental and important reference, it will be well to consider in what sense any mental process can lay claim to be for psychology an element of mental life. There is nothing obscure in this, and the author, after stating that by elements he means "the simplest materials out of which we are to build up our entire psychology" (p. 46), goes on to say that such materials, which are "actual items of mental experience" (p. 50), must be "strictly elementary, they must remain unchanged, however persistent our attempt at analysis and however refined our method of investigation" (p. 46). Indeed, it is the business of psychology "to describe and explain these elementary processes, and to show that, when grouped and arranged in certain uniform ways, they give rise to the different complex processes that constitute human consciousness" (p. 48). Such an elementary process the image is affirmed, by Titchener, to be. He writes: "Images are, in just the same way (as sensations), the characteristic elements of ideas, of the mental pictures that memory furnishes of past and imagination of future experience." Now if we ask what the nature of such an image is, we are told that the "image differs from the corresponding sensation in three respects, its qualities are relatively pale, faded, washed out, misty, and its intensity and duration are markedly less" (p. 198). These differences are, however, "differences of degree, and not of kind"; and if we ask the reason for this the answer is, as the passage suggests, that there is a correspondence between sensation and image and, as is stated elsewhere, the image arises later than its corresponding sensation, that is, is in some way dependent for its existence on the prior existence of the sensation. Here there is a tendency to assimilate the image with sensation, a tendency that has been current since the time of Hume. In the language of Külpe, what Titchener asserts is that the image is a centrally excited sensation, the sensation, of course, depending upon peripheral processes. As a further statement of opinion, the following passage is important: "But the writer is not sure that the image does not, as a rule, evince a sort of textual difference from sensation, that it is not more filmy, more transparent, more vaporous" (p. 199). This passage, we confess, is difficult to interpret. Moreover, its context does not clearly indicate its probable meaning. It occurs in a discussion of the question why, since the image differs in degree and not in kind from sensation, we do not more often confuse the image with sensation, if that is the fact, and the answer



is that this must be accounted for, in large measure, by "the differences of conscious context or setting in which the two processes occur." Then follows, by way of illustration, the statement that images are "less localized than sensations; they change and shift more rapidly and in a meaningless way; they move with the movements of the eyes. "But," the text continues, "the writer is not sure that the image does not, as a rule, evince a sort of textual difference from sensation, etc." What does this "but" mean? Does it mean that after all we are not dependent for our distinction between sensation and image on the criteria by which they are marked off as differing in degree and not in kind, but that we may find our criterion in the image character itself? A passage seeming to support this interpretation occurs on p. 365. If, then, it means this are we to suppose that this is true only within the "conscious context or setting," or does it apply also to the image under the standard conditions that determine its nature as an elementary process? If the former, it is not a statement that applies to the elementary imaginal process, but, as turns out later, to the images of memory and imagination; if the latter, an intrinsic difference has been pointed out, but we do not know whether this takes the place of the extrinsic distinction that occurs on the preceding page. Because, then, of the uncertainty of the conditions under which the statement is held to be true, we are in doubt whether the image has the sensuous qualities that assimilate it to sensation, or whether it has the textual difference that makes it intrinsically distinct from sensation. And if the author refuses the alternative and accepts both statements, it still remains to be shown how each of them can be true of an elementary process.

A great deal in Titchener's view of the image doubtless reminds the student of the history of psychology of the very similar way in which Hume has dealt with the image, and he will be inclined to recall the historic statement of the British empiricist that the image "strikes the mind" in a way different from sensation. He will also doubtless recall the passage in Stout's *Manual* (p. 397) where it is suggested, correctly probably, that by this phrase Hume intended to draw a qualitative, and not merely a quantitative, distinction. Whether this is consistent with the thoroughgoing sensationalism of the *Treatise* need not be discussed; but one is a little surprised and perplexed to find that Titchener does not avoid but aggravates the difficulty of the earlier writer, and we have to leave the former's treatment without any clear notion as to whether, as an elementary

process, there is any such thing as an image. His first passage tells us that there is; his second presents the image in a character which, in his own meaning of the term, is not "strictly elementary"; and his third passage raises a doubt as to both the other two. This illustrates, in another connection, the vascillation in statement if not in view that has been already referred to. For as we have seen above after assimilating the given process (the image) to sensation, something occurs in the exposition to remove it from this connection and to give it a quasi-independent standing. Until we know whether these later passages point out an intrinsic or extrinsic difference, we do not know how seriously we are expected to take the image as an elementary mental process.

The last of the three main passages referred to above occurs in the section "The memory-image and the image of imagination" (§118). We include the passage here because in the section the author states that the paragraph has to do with the elementary image. He writes: "It is clear, from what we have learned of the imaginal complexes in memory and imagination, that the elementary process, the image of §61, has two distinct forms . . . the image that may be confused with sensation" and "the image that is of filmier texture than sensation" (p. 420). This statement appears only to repeat a distinction already drawn on p. 199, and this we have considered. There is, however, this difference that whereas the author, in the earlier place, was stating the relations that the image has to sensation, in the latter he arrives at the statement through a consideration of the characteristics of the image of memory and imagination. All the difficulties, therefore, of the first passage press upon us here, unless the term "forms" is to be taken as an admission of the elementary image in a double character. If this is what is meant, it is not clear how this will comport with what Titchener says of the conditions that any process must fulfil to be an elementary process of mental life. When, however, in the immediate context we are told that these images appear respectively in the images of imagination and memory, we wonder whether this is intended to do away with the distinction between the elementary and developed processes of the imaginal type. That is to say, if images of imagination and memory are forms in which the elementary imaginal process exists, how can it be true, as we saw above, that this latter is "made into" the former by the supervention of characteristic emotive processes? The elementary image, thus, comes before us once again in an equivocal character, and we may repeat that until the status of the

elementary image has been fixed, it were fruitless to take *au sérieux* the author's theory of memory and imagination.

If this paper were not already long, it would be possible to show that the main difficulties of the author's exposition are due to the conflict between two points of view which are not always distinguished or under control. We cannot avoid the impression that while structural motives are uppermost, genetic considerations are operating in many places and in important ways. To test this supposition, it would be necessary to examine in detail the various characteristics that have been assigned in the text to the image of memory and imagination (cf. pp. 417, 421, 424). We must remain satisfied with a partial reference to a single statement, and that a quite general one. The image of memory and imagination, we have seen, has been described respectively as "discursive" and "integrative," and the author adds that "the consciousness in which the memory-idea (p. 413) and the idea of imagination (p. 422) is set may share the pattern either of primary or secondary attention," and attention for him is capable not only of a physiological but a genetic statement (pp. 271, 272). By genetic in this connection is meant that racial history has in important respects determined the characteristics under consideration. The more careful statement on p. 273 recognizes the influence of education in addition to that of heredity in determining the specific reactions. Now under education we must include not only the modifications of racial inheritance by acquired reactions, but also the results of such modifications as are seen in those cases where significance for the individual comes to attach to particular concrete environments. To take Titchener's example, the idea of working for an examination is the important focus of a complex situation—in which the tendency to follow a fire engine appears as a disturbing factor—for the student. It is not so for the man who, although he were working for an examination, happened at the same time to belong to the fire department. In this case there would be, through interest or what not, a change in the total situation which, to the onlooker, would be identical in the two cases. We refer to these possibilities because they emphasise the importance of taking into account elements and factors of problems that are given only a onesided statement in a purely physiological or experimental psychology. It is regard for these factors that gives to genetic psychology its distinctive task. For genetic psychology can take neither the environment apart from the subject, nor the subject apart from the environment, but considers environmental and sub-



jective conditions as continuous and interrelated factors in each mental complex. This statement is illustrated in Titchener's treatment of the "discursive" and "integrative" characteristics of the imaginal consciousness, for as we saw, these characteristics are due to the movements of the attentive, imaginal, and associative factors present in this type of experience. Memory and imagination are mental complexes whose character is to be read in the rôle played by the above-mentioned elements under the conditions that are present in the specific case. But when the patterns of attention are carried over into the memory and imaginative consciousness, these latter are definitely set before us as problems in genetic psychology. And if this is true, we have a point of view for interpreting those features of the doctrine that involve the idea of development, those, for example, that have been summarized in the second table above. In particular, the assertion that the image is "made into" an image of memory and imagination, and that the "typical forms" of each of these "shade off" into the other forms of the conscious experience, are, if they are anything, genetic statements; but they do not seem to have been brought, in the exposition, into explicit and clear relation with the point from which the discussion starts out and which is concerned to maintain the primary elementary character of the image. We are, consequently, left with this confusion: either the image fulfils the requirements of an elementary process and then it cannot become anything else; or, the image undergoes characteristic developments and then it is not elementary in the author's meaning of the term.

We may refer to one other point in conclusion. The author says that there is a continuity between the "typical forms" and the "genetic forms" of the memory and imaginative consciousness in the sense that when the one "shades off" into the other a "psychological circle" is formed (pp. 414, 422). Does this mean that if we start at any point in the imaginal consciousness, say with remembrance, it is possible by a series of progressive changes to run our line through all the other forms without a break? If this is what is meant, does it not imply a distinctively genetic view of the imaginal consciousness? But if this is the meaning, we have to confess that we have failed in our attempt to do this, and it would be interesting to know whether Titchener has been more successful. If he has, we should have to acknowledge his genetic view, but should have to maintain that his conception of memory and imagination is, in important respects, defective, and that parts of his exposition are irrelevant. For we again insist that if the account is genetic it is

difficult to see what part the elementary image has to play in the process; and if it is genetic in the sense of being circular, the account is defective in not recognizing the divergence between a memory and an imaginative consciousness.

We close the discussion with three remarks: (1) Where structural motives are in control, the author shows minute knowledge and clear appreciation of the problems in hand, and (2) where genetic considerations are uppermost there is much with which the writer is in cordial sympathy. But (3) when we try to find out the relations between the foregoing points of view, the *Text-book* fails to give the proper guidance, and this reacts unfavorably upon the statement of the author's views.

ARTHUR ERNEST DAVIES

OHIO STATE UNIVERSITY

## MEMORY AND IMAGINATION: A RESTATEMENT

Professor Davies has allowed me to read the manuscript of his critique; and the Editor has invited me to reply in the present number of the REVIEW. These are courtesies which I gratefully acknowledge. Psychology is still in so unsettled a state, that everyone who tries his hand at systematization must welcome criticism. When, however, the criticism deals less with scientific method and result than with interpretation of a text; and when the critic writes from a point of view which is foreign to the author criticized; then it seems but just that the reader should have the two statements, objection and rejoinder, laid before him at the same time. I am therefore glad to avail myself of the opportunity, which Professor Davies and the Editor have kindly afforded me, to discuss certain passages of my *Text-book*.

I begin with the Image as psychological element. The *Text-book* is intended for use in the class-room; it is meant, that is, to be read in order, chapter by chapter; and its later sections are always to be understood in the light of the earlier. The first section that deals with the imaginal element is §10. Here I point out that psychologists are not yet agreed upon the nature and number of their elements; that there is, however, a fairly definite trend of opinion; that we may therefore proceed, in the *Text-book*, upon a certain assumption; and that, if the future brings change, this will probably be a change by addition and not by subtraction. The assumption is that there are at most three elements; that two of these (sensation and image) may be considered as sub-classes under a general heading, even if they may not be grouped outright in a single class; and that all three (sensation, image, affection) may plausibly be reduced to the same ultimate type. I then characterize images, in a sentence, as the elements of ideas; I say that they are often confused with sensations; and I refer forward to §61.

I have, then, "affirmed the image to be" an elementary process; but I have not, at this stage, affirmed it to be an elementary process distinguishable, in psychological analysis, from sensation; on the contrary, I throw out the suggestion (new to the beginner in psychology) that sensation and image, the characteristic elements of perception and idea, are not seldom confused. When the issue is



expressly raised, in §61, I say—not, as Mr. Davies makes me say, that “the image differs from the corresponding sensation in three respects,” but something very different; my statement is: “It is usually said that the image differs, etc.” I thereupon proceed, on my own behalf, to argue that the alleged differences, being differences only of degree, cannot be admitted as final; and I show that, in fact, they are not adequate to a differentiation of image from sensation. Why, then, I ask, do we not confuse image and sensation in daily life? I reply, first, that we probably do confuse them far more often than we realize,<sup>1</sup> and secondly that we may in many cases distinguish them, not by intrinsic difference, but by difference of conscious context and setting. So far, therefore, I still favor the identification of sensation and image. But now comes the mention of a possible intrinsic difference. “The writer is not sure that the image does not, as a rule, evince a sort of textural difference from sensation; that it is not more filmy, more transparent, more vaporous. If this is the case, then it is better to consider sensation and image as sub-classes of a particular type of mental element than to include them outright in a single class.” “This passage,” says Mr. Davies, “is difficult to interpret.” Yet it is, I think, susceptible of only one interpretation. I recommend the reader not to identify image outright with sensation; there is some evidence of an intrinsic, attributive difference between the processes,—a difference which I do not name, since naming would be premature; but which, if its existence is verified, will have to be called ‘modality’ or ‘substantiality’ or something of the sort. The discussion of the ‘relations’ of sensation and image (p. 48) thus ends in a *non liquet*; though I indicate—if the personal matter is of any importance—that I am more disposed to separate sensation from image than I was when I wrote the *Outline*. The elementariness of the image, of course, is nowhere called in question.

I come now to §118, which is based upon the results of Perky’s experiments. I confess that these experiments gave me a good deal of trouble. I believed that their conclusion was valid; but I foresaw that it would not be generally accepted without confirmation from other quarters; I realized that suspense of judgment was scientifically justified; and I felt that I had therefore no right, in a text-book, to press the conclusion to its logical consequences.<sup>2</sup> What I did was

<sup>1</sup> This point is illustrated further on, in the fine-print paragraph of p. 365, where a back-reference to the present p. 199 is given. There are, of course, no text-references from coarse to fine print; but the Index covers everything.

<sup>2</sup> Professor J. R. Angell has, in fact, taken me to task for admitting the experiments (*Philosophical Review*, XX., 1911, 547). But in such cases a writer must use his own judgment.

to give the outcome of Perky's work, in fine print (pp. 420 f.), and to leave the reader to draw his inferences as he pleased. It is clear, I say, that the experiments give us two elementary 'images,' two kinds of 'characteristic elements of ideas.' The one is indistinguishable from sensation; it is imaginal only in the sense in which popular psychology speaks of images; in strictness, it is not separable from sensation. The other is "of filmier texture than sensation,"—an obvious reference to §61. I try to account, physiologically, for the first kind of 'image'; but there I stop. And I fail to see in the exposition anything like obscurity or contradiction. The coarse print of §61 says: 'Image is often confused with sensation, but still there may be an attributive difference: keep your minds open.' The fine print of §118 says to the instructor: 'Here are the experiments (417 f.), and here is what they tell us of the elementary image (420 f.); they bear out what was said in §61 of a possible intrinsic difference from sensation (the element of the imaginal complex in memory), while they also account for the confusion with sensation (the element of the imaginal complex in imagination).' If the instructor accepts the results, he will make his teaching more positive than §61; if he does not, he will offer his criticisms, and allow §61 to stand as written.

I hope that I have herewith met Mr. Davies's difficulties with regard to the elementary image. I find two, plainly distinguishable elements: sensation and affection. I find another alleged element, the image; this is set off from sensation, in the books, either by reference to a central origin or by difference of attributes. The origin is of no concern to descriptive psychology; and a comparison of attributes leaves us in doubt whether we should operate with sensation alone or with sensation and image as distinct processes. If, now, we accept certain experimental results, and interpret previous experimental work in the light of these results, the situation clears: what has been called the 'elementary image' turns out to be two things, an elementary process indistinguishable from sensation, and an elementary process modally different from sensation. The former 'image' is, for systematic psychology, a sensation; it disappears as image from our discussion; the technical term 'image' is reserved for the second type of process.

Further than this, I cannot "fix the status of the elementary image"; the 'if' is there, and cannot be got rid of. I proceed to the psychology of Memory and Imagination.<sup>1</sup>

<sup>1</sup> Mr. Davies entitles his paper "Professor Titchener's Theory of Memory and Imagination." I am, however, but little occupied with theory; what I attempt is a descriptive psychology of the two topics.

In §117 (which comes before §118, and therefore depends for its doctrine of the elementary image upon §61) I begin with the statement that "no image or idea is intrinsically a memory-image or a memory-idea." Neither does analysis reveal a specific element 'memory-image,' nor is any imaginal complex in its own right a 'memory.' Ideas are 'made into' memories by a context; not genetically, of course,—I do not raise that question,—but descriptively, as a matter of analysis and synthesis. The context itself is in memory what it was in recognition; "the sole difference" between the two consciousnesses, in cross section, is that "the focal process is an idea and not a perception." In recognition (§115) the contextual 'feeling of familiarity' may be regarded as a particular derivative of the secondary effects of sensory stimulation; these effects, in general terms (§60), are associative processes, organic sets and attitudes, and affective processes. I do not attempt to describe their nervous correlate. Mr. Davies now asks: How are the organic set and the affective process aroused in the case of memory? And the question, if I understand it aright, means: Are they aroused directly, as secondary effects of the central stimulation, or only indirectly, by way of association? It is, in other words, a question of nervous correlate. As such, it does not concern me; and I do not remember that I considered it, when I wrote the section. It is, however, an interesting question, because psychologists, when they are dealing with centrally excited processes, usually take the associative mechanism as a matter of course. I think—if an opinion given without much time for reflection is worth anything—that the associative mechanism (in the sense of §§105–111) is adequate to the memory context; and I think there is experimental evidence to show that the organic reaction to perception is, other things equal, at any rate more widespread and more intensive than the reaction to idea. But since central stimulation is still stimulation, and the organism is still a system, I should imagine that secondary effects of some range and of some intensity are inevitable.

I recognize no memory other than that given in remembrances and recollections; I believe, however, that memory, like recognition, may be definite or indefinite (pp. 409, 413). In the same way, I recognize no imagination (§119) other than that given in particular cases of imagination. And as with memory, so with imagination: an imaginal complex is 'made into' an 'image of imagination' by a context, by a peculiar 'feeling of strangeness.' The opening paragraph of §119 is written with direct regard to the opening paragraph of §117. The terms 'discursive' and 'integrative,' which Mr. Davies finds



so disquieting, are meant—I was on the point of writing ‘of course’—to give a temporal, not an areal, characterization of the two consciousnesses. It did not occur to me, I admit, to attach the labels (“If we look at the consciousness in longitudinal section,” etc.); but I hardly think that, at this stage of the book, the labels are needed.<sup>1</sup>

There remains the “psychological circle” of pp. 414, 422, which Mr. Davies again views through his genetic glasses. My standpoint is still purely descriptive. I want to impress upon the student that the comprehensive terms of descriptive psychology are names of type-consciousnesses only, and that between the type-consciousnesses there are all manner of intermediate forms. Put memory, then, at the north pole of a circle, and imagination at the south pole. Starting, on the one side, from memory as remembrance, we pass through day-dreaming to reproductive imagination; starting, on the other side, from memory as recollection, we pass through search and thought to creative imagination; the former is the side of primary, the latter the side of secondary attention. The diagram serves to give the student his bearings among certain gross terms of current psychology, and also, by the continuity of the circle, reminds him that his everyday consciousnesses are probably not as clear-cut as the consciousnesses of his text-book. I do not think that much is gained by the elaborate figures and formulas which many psychologists use in their description of the ‘higher’ processes; but this particular diagram is very simple; and, if it is not necessary, I hope at least that it is harmless.

I have now replied to what I take to be the essential points of Mr. Davies’s criticism. The criticism is, throughout, of the immanent kind; my exposition of the psychology of Image, Memory and Imagination is considered as a whole, without regard to pedagogical order; and, aside from the reference to Hume and Stout, the experimental and historical sources from which my data are obtained do not come

<sup>1</sup> Here, *e. g.*, are two sentences from p. 423. “In memory, the observer is always within a certain universe of discourse; there are limits, set by the fixity of past occurrence, which he may not transgress, but within this breadth of context he can move at will; consciousness is discursive. In imagination, consciousness proceeds, as a whole, from the fountain-head of disposition; there are no limits of any kind, save those of individual capacity and experience; but the stream, whatever its volume, flows always in a determinate direction; consciousness, as we have said, is integrative.” I must add that there is no passage in my book where “the image of memory and imagination . . . has been described respectively as ‘discursive’ and ‘integrative.’” I should suppose that Mr. Davies had here made a mere slip of the pen, were it not that other remarks regarding these ‘images’ are equally foreign to my intention and expression.

under discussion. Such criticism is necessary, so long as we have 'schools' of psychology; and I gladly acknowledge the scientific fairness of Mr. Davies's essay. On the other hand, I think that my critic's personal views have prevented a just appreciation of my own position. For the gist of his criticism, after all, is simply this: that I have not written of Memory and Imagination as he himself would write; that I leave things out which he would include; that I emphasize features of the two consciousnesses which he regards as of minor importance. But then, so far as one may judge from articles, Mr. Davies's psychology is very different from mine. And reading the *Text-book* in the light of his own system, he has, perhaps, found difficulties and obscurities which would not arise either for a more sympathetically minded psychologist or for the beginning student.

E. B. TITCHENER

CORNELL UNIVERSITY





# THE PSYCHOLOGICAL REVIEW

---

## THE CURVE OF WORK

BY EDWARD L. THORNDIKE

*Teachers College, Columbia University*

Kraepelin and other students of the changing efficiency of a mental function under continuous exercise have analyzed the gross course of efficiency into certain supposed features or elements. These are the practice effect, the fatigue effect, the 'warming up' effect (Anregung), adaptation (Gewöhnung), initial spurt, end spurt, spurts after fatigue (Ermüdungsantriebe), spurts after disturbance (Störungsantriebe) and the rhythm of attention.

They also often assume that the names thus given to certain changes in efficiency signify adequate causes of the changes. For instance, in distinguishing as 'Gewöhnung,' a rise in efficiency, slower than that which they would call 'Anregung' and less permanent than that which they would call the effect of 'Uebung', they commonly assume that some real thing, Gewöhnung, exists, which is essentially different from the other real things, Anregung and Uebung.

I shall in this paper use these terms only in the former objective meanings of changes in the efficiency of the function, asking, for instance, to what extent initial spurt—a high degree of efficiency appearing in the few minutes of work—is characteristic of work curves in general, or of certain individuals in certain kinds of work.

### INITIAL SPURT

This phenomenon is certainly not characteristic of work curves in general. In the case of the 37 work periods of 16

subjects engaged in mental multiplication (of a three-place number by a three-place number)<sup>1</sup> there was no evidence of it. In the case of five adults working at addition<sup>2</sup> (each for four two-hour periods), there is no evidence of it.

Since the results of this second series of experiments will be referred to in several connections, they may best be described now.

Educated adults, graduate students of psychology, worked at adding for  $1\frac{1}{2}$  or 2 hours as continuously as possible, recording the time at the completion of each row of 16 examples. 21 different rows, each of 16 examples, were used to avoid memorizing any answers or sequences. The experiment was repeated four times on different days and finally a test of 10 to 15 minutes length was made. Each of the four long series was made under substantially the same external conditions; the fifth test was always made after a full night's rest or more. I append a sample record in Table I. I have turned the gross scores into terms of a single variable by adding five seconds for each error.<sup>3</sup>

I shall present here the results from only five individuals. The results from the others are in agreement with every conclusion which will be stated.

I show in Figs. 1, 2, 3, 4 and 5 the work curves for the five subjects separately.

Since efficiency is measured inversely by the corrected time per row, the lower the curve the greater the efficiency. Since one important fact to be shown by these curves is the variation of the same subject from day to day, the four curves from any one subject are so scaled vertically that the average achievement per unit of time is approximately equal on all four days, and are so scaled lengthwise that each successive fraction of the time is represented by the same abscissa-length on all four days.

I call the reader's attention particularly to the great varia-

<sup>1</sup> The results of which are summarized on pages 75-80 of the *Journal of Educational Psychology*, Vol. II.

<sup>2</sup> The examples were each of 10 one-place numbers, printed in a very clear type  $3\frac{1}{2}$  mm. high.

<sup>3</sup> I have also all the results reduced to terms of a single variable by a much heavier penalizing of errors, namely, an addition, per error, of one fifth of the time for 16 examples. All the conclusions to be stated in this paper are supported equally by the results by either method of equating speed and accuracy.

TABLE I

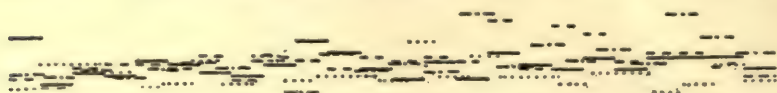
## SAMPLE RECORD OF GROSS SCORES

Time taken and errors made in successive rows of 16 ten-digit examples, on four days after approximately the same amount of general mental work and on a fifth day after long rest. Subject *Mc*.

Row	Beginning at 2 P. M., March 10		Beginning at 1:10 P. M., March 11		Beginning at 7:30 P. M., March 12		Beginning at 1 P. M., March 13		Beginning at 9 A. M., March 17	
	Sec.	Err.	Sec.	Err.	Sec.	Err.	Sec.	Err.	Sec.	Err.
1	150	1	138		185	1	172		110	
2	145	1	138		171		120		105	
3	145	1	133		171		110	1	105	1
4	133		122		173	1	119		100	1
5	133		131		172	1	105		105	
6	140		120		125		112	1	108	
7	135	1	115		110		110	1	100	
8	150	1	128		116		112		115	
9	153	2	132	1	130		115		95	
10	142	1	113		111		103			
11	160	2	124		115		118			
12	150	1	126		110		111			
13	140	1	118	1	112	1	105			
14	132		133	1	120	1	120			
15	150	1	135		130	2	127	1		
16	135	1	127	1	113		122	1		
17	157	1	128	1	109		124	1		
18	146		120		135		112			
19	143		133		117	1	115	1		
20	142	1	127		122		95			
21	168	1	128	1	106		125			
22	156		120		112		120			
23	158	1	120		116		111			
24	148		140		117		108			
25	138		138		118		106			
26	148		140		114		110			
27	147	1	135	1	123		112	1		
28	138		115		128	1	103	1		
29	147	2	145		138		120			
30	145		125	1	128	1	115			
31	137	1	135		110		118			
32	127		125		120	1	110			
33	137		130		130	1	115			
34	135	1	122		120	3	110	1		
35	133		132		117		108			
36	138		150	2	130		120			
37	122		132	1	138		116			
38	138	1	128		136	1	110			
39	132	1	144	2	122		113			
40	134	1			127		113			
41	130				133		110			
42	140				135		135			
43	130				127		120			
44	138				127		107			
45	130	1			122		125			



tion in the form of the work curve for any individual from day to day.<sup>1</sup> Among the daily records can be found some which



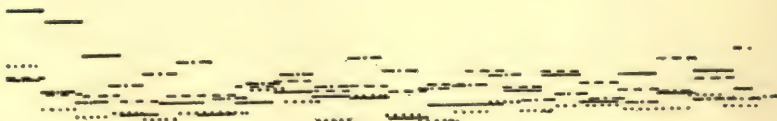

---

FIG. 1. Work Curves of Subject *D*. Four Days.




---

FIG. 2. Work Curves of Subject *L*. Four Days.




---

FIG. 3. Work Curves of Subject *Mc*. Four Days.

<sup>1</sup>The variation from day to day in the form of the curve of work may, in the case of one subject, *R*, be due in some considerable degree to the use of a too coarse unit in recording the time. But such is not the case with any other subject.

could be used, if taken singly, as excellent examples of initial spurt, end spurt, warming up, or fatigue, of various types. But such selection of a single day's record is obviously unjustifiable, since the same person on another day shows opposite fluctua-

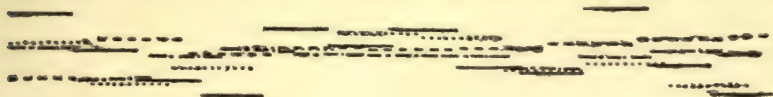


FIG. 4. Work Curves of Subject R. Four Days.

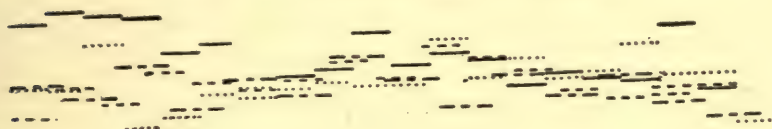


FIG. 5. Work Curves of Subject S. Four Days.

In Figs. 1-5 the long dashes all refer to the 3d day's work curve; the short dashes, to the 2d day's; the dotted lines, to the 4th day's; and the dash and dot lines, to the 1st day's.

tions.<sup>1</sup> Many different days' records are necessary to determine the forms of an individual's work curve.

<sup>1</sup> The inconstancy of the same individual in respect to the form of the work curve would not have been so great if the work had been that of saying to oneself or writing down the results of *each successive* addition, or if the subject had not expected a heavy penalty for an error, or if the subject had not pushed himself to the utmost, or if the subject had had a very great deal of special practice in adding at his maximal rate, but without trying to add by grouping. But in any case the variation in the form of the work curve on different days is so great as to require careful consideration of the unreliability of any determination based on only a few days' records. These unreliabilities have rarely been considered by Kraepelin and his followers, so that it is possible that the fluctuations to explain which they have invoked initial spurt, final spurt, spurt after fatigue, spurt after disturbance, or the rhythm of attention are one and all accidents of the unreliability of the determinations.

Initial spurt, if a real fact, will be found in an examination of the work, minute by minute, of the first quarter of an hour. Kraepelin supposes it to be a phenomenon chiefly of the first five minutes. It is possible to find two or three single records which might, if alone, be so interpreted, the most plausible case being one of subject *L*'s: 265 seconds for the first 16 examples (no errors), 335 seconds for the next 16 (2 errors), and 360 seconds (4 errors) for the next 16. But these single instances are demonstrably not due to any consistent initial spurt. Exactly opposite instances occur, and in the same individuals. The average time (plus 5 seconds for each error) for each subject, for the four days' trials of each successive row is given in Table II., including for each individual approximately his first twenty minutes' work in each of the four work periods.

TABLE II

Average time (corrected for errors) required to add successive rows of 16 ten-digit examples at the beginning of the four work periods. Five subjects—*B*, *L*, *Mc*, *R*, and *S*. Time in seconds.

Row	Subject				
	<i>D</i>	<i>L</i>	<i>Mc</i>	<i>R</i>	<i>S</i>
1	100	284	164	186	149
2	100	311	146	183	142
3	104	318	143	206	155
4	100	301	138	204	158
5	99		137	186	147
6	95		126	195	146
7	91		120		159
8	90		128		
9	113		136		
10	105				
11	96				
12	99				

Each 'row' equals 16 examples or 144 additions, involving the writing of 16 two-place numbers as answers, and one observation of the watch and record of the time-

The absence of any evidence of initial spurt in the facts of Table II. is better realized by presenting them graphically. Let each centimeter horizontally approximate five minutes, let the same vertical height represent approximately the average efficiency of the function for each individual. We then have approximately Fig. 6, which shows the absence of any uniform fluctuation of any sort.



Or we may go back to the original scores and compute the work for each successive five minutes for each individual. The results are as in Tables III. and IV. Weighting the results

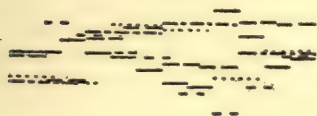


FIG. 6. Average Work Curves of the First 20 Minutes. Each Sort of Line (Dot, Short Dash, Longer Dash, etc.) Represents the Average Work Curve of One Subject.

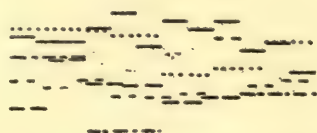


FIG. 7. Average Work Curves of the Last 20 Minutes. Each Sort of Line (Dot, Short Dash, Longer Dash, etc.) Represents the Average Work Curve of One Subject.

from all five subjects equally and averaging the five, we have the efficiencies for the four successive periods standing in the following proportions: 100, 99, 101 and 100.

TABLE III

Average number of examples done (each involving 9 additions, and the writing of a two-place number<sup>1</sup>) and of errors made, in each five-minute period of the first 20 minutes of work.

	<i>D</i>		<i>L</i>		<i>Mc</i>		<i>R</i>		<i>S</i>	
	Ex.	Er.	Ex.	Er.	Ex.	Er.	Ex.	Er.	Ex.	Er.
First 5 min...	48.8	1.7	16.8	1.5	31.3	.7	26.3	.7	33.6	1.4
Second 5 min.	49.9	1.0	15.8	1.7	34.8	.8	24.5	1.2	31.6	1.5
Third 5 min..	48.2	1.0	15.6	2.0	37.8	.7	25.5	1.9	33.5	1.7
Fourth 5 min.	48.6	.8	16.2	.7	37.4	1.1	24.9	1.9	31.5	2.3

<sup>1</sup> Because of the time required to write the answers and to record the time after every 16 examples, the number of additions that would have been made in mere addition may be taken to be not 9 but 10 times the numbers entered in the table.

TABLE IV

Per cent. which the efficiency in each successive five-minute period of the first 20 minutes of work is of the individual's average efficiency for the 20 minutes.

	D	L	Mc	R	S
First 5 min.....	100	104	89	104	103
Second 5 min.....	102	98	99	97	97
Third 5 min.....	98	97	107	101	103
Fourth 5 min.....	99	100	106	98	97

I am unable to find anywhere any evidence of consistent initial spurt with any individual in all mental functions or with all (or nearly all) individuals in any kind of mental work, much less with all individuals in all kinds of work. The work curves obtained by Oehrns,<sup>1</sup> Amberg,<sup>2</sup> Weygandt,<sup>3</sup> Lindley,<sup>4</sup> and other workers in Kraepelin's laboratory give no such evidence. Nor is it to be found in the data got by Yoakum,<sup>5</sup> so far as he presents them.

Lindley's work, which was the most extensive, showed as speed ratios for successive five-minute periods at the beginning of work, 100, 98, 97, 97, and 96, using the data from all three subjects. The first five minutes, that is, differed from the second substantially only as the fourth from the fifth. Putting together Weygandt's series I find ratios of 100, 97 and 95½ for the first three five-minute periods. Hoch showed, on the whole, ratios of 100, 99, 98 and 94 for the first four five-minute periods. Miesemer<sup>6</sup> showed ratios of 100, 96, 98, 97. In fact the very results of Rivers and Kraepelin,<sup>7</sup> to explain which Initial Spurt was specially invoked, give as ratios (by five-minute periods) 100, 87, 99, 101, 102, 102. Clearly the fact in them to be

<sup>1</sup> Oehrns, A., '95, 'Experimentelle Studien zur Individualpsychologie,' *Psychologische Arbeiten*, Vol. I., pp. 92-151.

<sup>2</sup> Amberg, E., '95, 'Ueber den Einfluss von Arbeitspausen auf die geistige Leistungsfähigkeit,' *Psychologische Arbeiten*, Vol. I., pp. 300-377.

<sup>3</sup> Weygandt, W., '97, 'Ueber den Einfluss des Arbeitswechsels auf fortlaufende geistige Arbeit,' *Psychologische Arbeiten*, Vol. II., pp. 118-202.

<sup>4</sup> Lindley, E. H., '00, 'Ueber Arbeit und Ruhe,' *Psychologische Arbeiten*, Vol. III., pp. 482-534.

<sup>5</sup> Yoakum, C. S., '09, 'An Experimental Study of Fatigue,' Psychological Monograph No. 46 of the *PSYCHOLOGICAL REVIEW*.

<sup>6</sup> Miesemer, K., '02, 'Ueber psychische Wirkungen Körperlichen und geistiger Arbeit,' *Psychologische Arbeiten*, Vol. IV., pp. 375-434.

<sup>7</sup> Rivers, W. H. R., and Kraepelin, E., '96, 'Ueber Ermüdung und Erholung,' *Psychologische Arbeiten*, Vol. I., pp. 627-678.

explained is the 87 of the second five minutes rather than the 100 of the first five.

It is, I admit, very likely that some individuals in some kinds of work tend to fall off rapidly from the too exacting standard which they set themselves at the beginning, just as some tend to rise rapidly above the standard which they cautiously try at the beginning. But these idiosyncrasies must not be misinterpreted as a general law. Until some hypothesis can be framed about initial fluctuations which will enable one to *prophesy* the form of the work curve, if only for a single individual or for a single function, these idiosyncrasies belong under the heading of 'accidental' or 'chance' variations—that is, of fluctuations whose causes are unknown.

### END SPURT

It is often the case in ordinary mental work with a time limit, that as one approaches the end of the work period, the knowledge that he is approaching it leads him to spurt. In ordinary mental work one does not work throughout at one's possible maximum, so that such a spurt is easily possible. In experimental work, when the subject is required to work throughout at maximum possible efficiency, such a spurt can come only if the subject has deliberately disregarded instructions, or if the knowledge of the approaching end releases forces over which he had no control. The latter is apparently possible, various external stimuli, such as other competing individuals, applause and the like, being apparently able to add a reinforcement beyond what a subject's own determination can summon.

How common an occurrence such a final spurt is in work carried on with no deliberate reservation of power, and of how great a magnitude it is, may be estimated<sup>1</sup> from the following facts:

It is not common or important enough to make the last eighth of a two-hour period one per cent. more efficient than

<sup>1</sup> For a demonstration of the existence and amount of end-spurt one should have subjects work to the end of a given period with and without knowledge that the end was approaching, all other conditions being identical.



the average eighth in the ten individuals each measured in six sorts of work by Oehrn.

In 16 subjects working for very long periods (4-12 hours) the effect of end-spurt in those who worked to a time limit is not great enough to counterbalance the effect (real or possible) of the decision of some to stop work because of having made a series of very inferior records.

In the four two-hour tests in addition previously described, not one subject of the five showed a *consistent* tendency to increase his efficiency by even five per cent. during the last 5 or the last 10 minutes. For the five subjects whose records have been computed in detail the facts are given in Table V. and Fig. 7. (In Fig. 7 as in Fig. 6, each centimeter

TABLE Va

Average time (corrected for errors) required to add successive rows of 16 ten-digit examples at the ends of the four work periods. Five subjects—*D*, *L*, *Mc*, *R*, and *S*. Time in seconds.

	<i>D</i>	<i>L</i>	<i>Mc</i>	<i>R</i>	<i>S</i>
Eleventh from last row added	105				
Tenth from last row added...	104				
Ninth from last row added...	104				
Eighth from last row added...	108		128		130
Seventh from last row added...	113		130		134
Sixth from last row added...	103		126		148
Fifth from last row added...	110		125	205	142
Fourth from last row added...	108		126	205	141
Third from last row added...	110	297	143	203	137
Second from last row added...	101	254	129	188	138
Next to last row added...	104	365	130	190	110
Last row added.....	95	278	134	200	126

TABLE Vb

Average time (corrected for errors) required to add successive equal amounts (3 rows for *D*, 1 row for *L*, 2 rows for *Mc*, 1 row for *R*, 2 rows for *S*) at the ends of the four work periods.

	<i>D</i>	<i>L</i>	<i>Mc</i>	<i>R</i>	<i>S</i>
Fifth from last.....				205	
Fourth from last.....				205	
Third from last.....	313	297	256	203	282
Second from last.....	324	254	251	188	283
Next to last.....	328	365	272	190	275
Last.....	300	278	264	200	236

horizontally approximates 5 minutes; the same vertical height is used to represent approximately the average efficiency of

the function for each individual.) There is a probable tendency to fluctuation in the direction of 5 per cent. greater efficiency in the last few minutes, but the central tendency is almost certainly not to a change of over + 10 per cent., and is perhaps to as little change as 0.

Amberg's average results at the end of one- or two-hour periods of adding were, by five-minute periods, in the ratios 100, 101, 101 and 96. Weygandt's average results in the last twenty minutes of 90 minutes' continuous adding were in the following ratios:

Subject A.....	102	95	100	107
Subject B.....	100	99	99	100
Subject D.....	100	99	102	98
Average.....	101	98	100	102

Of Lindley's three subjects one did a bit more, one a bit less, and one almost exactly the same amount of addition per minute in the last 5 as in the average of the last 20 minutes. Combining the data for all three there is not a difference of one per cent. Miesemer's results for the last four five-minute periods of an hour's adding were in the proportions 101, 100, 99, 106.

On the whole, no subject who has been tested four or more times shows consistently any considerable end-spurt and the general tendency is to a rise, in the last five or ten minutes, of not over three per cent.

#### SPURT AFTER FATIGUE AND SPURT AFTER DISTURBANCE

In mental work in ordinary life a person may obviously, if he is not doing his best, at any time do a little better to make up for an observed temporary deficiency however caused. Deficiencies due to disturbances certainly, and to fatigue, if that acted unevenly throughout a work period, might be thus noticed and counterbalanced. In a subject who is keeping his efficiency at a maximum so far as he can control it, the observation of a fall in achievement might still so act as a reinforcement. Kraepelin and his followers assume that it does, and seem to regard each 'drop-rise' sequence in the

curve of work as a deficiency caused by fatigue or disturbance which stimulates a gain in efficiency as a result of an Ermüdungsantrieb or Störungsantrieb.

It should be noted, however, that on general grounds the suggestion that one is doing well would seem more favorable to the efficiency of one already doing his best than the suggestion that he is doing badly, and that empirically no one has correlated the fluctuations in work curves with the incidence of disturbances of known character. The doctrine of spurt after fatigue and spurt after disturbance in the case of work done under the conditions of the ordinary fatigue experiment, is then at present a speculative hypothesis. It was devised apparently to explain the fluctuations in efficiency, from one minute or five-minute period to another, which are found in continuous adding, cancelling letters, memorizing, or other forms of mental work.

A rise following a fall in the curve easily attracts observation and tempts readily to theorizing. A rise followed by a rise or a fall followed by a fall is not so striking. The explanation of a 'fall-rise' sequence by spurt after disturbance or spurt after fatigue is really unwise, however. For if the fall is caused by a disturbance, no cause is required for the rise save the ending of the disturbance; while if no external cause is known for a given fall, there is no reason why one should pretend to know the cause of its sequent rise. The wiser effort would be to seek hypotheses which would account for rise-fall, rise-rise, fall-fall, and fall-rise sequences, one and all, and, until such hypotheses could be subjected to verification, to be content with attributing them to 'accidental' variations.

### RHYTHM OF ATTENTION

I have no new data to report bearing on the theory of Voss<sup>1</sup> that efficiency in adding fluctuates in a rhythm of 2 up to 3 seconds. But since Voss's work has been referred to approvingly by later writers, it may be worth while to show its essential unsoundness.

<sup>1</sup> von Voss, G., '99, 'Ueber die Schwankungen der geistigen Arbeitsleistung,' *Psychologische Arbeiten*, Vol. II., pp. 399-449.



Voss's study ('99) is instructive as a sample of the difficulty of keeping in mind just what a set of measurements means while one is arguing from them. What he does is in essence as follows:

Suppose the times of a series of additions are, in order, in fifths of a second, 3-3-6-5-3-8-3-1-3-1-3-4-3-3-3-6-2-3-3-5-4-5-3-3-2-4-2-3-3-4-2-7-3-3.

What Voss calls the duration length of a fluctuation is such a time as, in this case: 3 plus 3 plus 6; or 5 plus 3 plus 8; or 3 plus 1 plus 3 plus 1 plus 3 plus 4; or 3 plus 3 plus 3 plus 3 plus 6; or 2 plus 3 plus 3 plus 5; or 4 plus 5; or 3 plus 3 plus 2 plus 4; or 2 plus 3 plus 3 plus 4; or 2 plus 7. That is, Voss divides the entire work into a series of lengths of time called fluctuations—in this case 12, 16, 15, 18, 13, 9, 12, 12, 9—each ending with an addition slower than any since the end of the last fluctuation. The length of each fluctuation is then always the sum of *two or more* single addition times.

Now, this being the method of scoring and the time of three fifths of a second being the most common, differences of three fifths of a second in the fluctuations *must* be the most common differences. The case is as if one reckoned fluctuations in the income of a man whose salary was almost always raised or lowered by approximately \$100.00. The fluctuations would of course almost always vary by steps of approximately \$100.00.

Voss, finding such a variation, hails it as important and discusses seriously explanations of this, to him, very remarkable phenomenon, by attention waves and other features of the worker, but finally observes that it is due to the fact that by the artificial conditions of the test and method of scoring one fluctuation must differ from another in length by the length of one, two or more additions.

He clings, however, to the faith that the great frequency of fluctuation lengths from 10 to 13 fifths of a second has some real and unitary cause in the constitution of the worker. He considers the relation to the respiration curve, and to the optimum time between the warning signal and the stimulus in experiments in reaction time, and concludes that the great frequency of fluctuations of 10 to 13 fifths of a second is due

to the fact that 'the attention tends to rise to its highest intensity (*Spannung*) in periods of somewhat over 2 seconds,' so that 'the most frequent length of a fluctuation expresses exactly the length of fluctuations of attention found in other investigations!'

There are two flaws in this argument. First, the length of fluctuation which Voss measured is not the length from a point of maximum efficiency to the next maximum nor from a minimum to the next minimum. Such lengths can be found only by somehow defining 'maximum' (say as an addition of 2/5 seconds or under) and 'minimum' (say as a time of over 5/5 seconds), and by them measuring the intervals. Voss's measures are of the intervals between *all* turning points. Such sequences as 1-2-1, 2-3-2, 6-7-6, or 7-8-7 give end-points for his fluctuations as truly as do the sequences 1-3-5, 4-1-2-6 and 3-1-1-5 in the series 6-1-3-5-4-1-2-6-3-1-1-5.

In the second place, the variation of single addition-times being around such modes as he indicates with such frequencies as he indicates, their appearance in an order *absolutely random* as respects speed, would give substantially as great a preponderance of fluctuations of 10-13 fifths of a second as he found. It seems to have been a misunderstanding of the laws of probability that led Voss to think that such a random order would make fluctuations of 7 fifths of a second the most frequent. It certainly would not.

Since Voss does not give the required data, I cannot calculate exactly what the relative frequencies of different lengths of fluctuation would be by mere chance, but they would certainly show a clustering closely corresponding to that which he explained by the rhythm of attention. I take at random his experiment VIII. Suppose the time for the separate additions to be such that for every addition taking 9 fifths of a second there occurred one taking 8 fifths of a second, two taking 7, three taking 6, six taking 5, eleven taking 4, 73 taking 3, and three taking 2. This is very close to the distribution in Voss's experiment, given on page 409 of his report. Suppose the order of occurrence of these times to be absolutely random. Then about a third of all the 'fluctuations' as defined by Voss

would be from 10 to 15 fifths of a second inclusive. This is the fact which he finds (on page 425 of his report) and from which he reasons to a 2-3 second rhythm with the attention-wave as a cause! The same will be found to hold for all the results of the experiments reported by this author.

### WARMING UP

The best definition of 'warming-up' as an objective act is that part of an increase of efficiency during the first 20 minutes (or some other assigned early portion) of a work period, which is abolished by a moderate rest, say of 60 minutes. Such warming up should show itself clearly in individuals at or near the limit of practice, and, in others, should compound with the effect of practice to make the rise in efficiency especially rapid in the first twenty minutes of work, or the fall (supposing the function to diminish in efficiency) specially slow in this same period. What time is assigned in the definition of warming-up effect is of little consequence to the investigation so long as *some* time is assigned.

In the case of my five subjects in addition it is clear from Figs. 1-5 that such objective warming-up either vanished within very few minutes or was so slight as not to appear consistently and not to appear at all in any considerable magnitude.

There is little or no direct evidence of warming-up in the records got by Oehrns, Lindley, Weygandt, Bolton,<sup>1</sup> Miesemer, or Rivers and Kraepelin. My sixteen subjects working at mental multiplication of a three-place by a three-place number showed signs of its presence, but not conclusively.

Since Oehrns's results are important for the consideration of both warming-up and fatigue, and since they were, like those of my five subjects, for two-hour periods, I give later a brief graphic summary of them. It will be seen from Figs. 8, 9 and 10 that, if a slight allowance is made for the greater effect of early than of late practice, no special warming-up effect is needed to account for the very slightly less decrease in efficiency during the first twenty minutes than during the average twenty minutes.

<sup>1</sup> Bolton, T. L., '02. 'Ueber die Beziehungen zwischen Ermüdung, Raumsinn der Haut und Muskelleistung,' *Psychologische Arbeiten*, Vol. IV., pp. 175-234.



It seems certain, however, from the cruder observations of daily life, that for many individuals in many functions, there is a warming-up effect as defined, but I am unable, with the data at hand from Kraepelin's pupils or others, to separate out this temporary improvement that comes at the beginning of the exercise of a function after rest, from the more permanent improvement that comes from exercise of the function in general. Experiments near the limit of practice are necessary. One must be careful not to mistake the sudden gain from practice in its early stages for warming-up. There is a temptation, for example, to see evidence of the latter in the first day's curves for my five subjects (shown in Figs. 1-5). But the absence of any similar gain in the first half hour in the curves for the last day for these same subjects proves that its presence on the first day was probably a result of practice.

It should also be noted that intellectual warming-up in the popular sense refers rather to fore-exercise of *other functions*, in order to get materials and motives with which and by which the given function is to work, than to an intrinsic alteration of it. Such is the case, for example, with 'poets, artists and composers' of whom Mosso<sup>1</sup> speaks.

#### ADAPTATION (GEWÖHNUNG)

Adaptation is, so far as I can see, definable objectively only as a slower, longer and somewhat more permanent warming-up effect. I am unable to identify or measure it in the accessible work-curves.

#### FATIGUE

Fatigue can be best defined objectively as that diminution in efficiency *which rest can cure*. The fatigue caused by a given amount of work without rest is then measurable by the difference between the efficiency at the end of that work period and that found after a period of rest plus, if necessary, a period of warming up. Since, in the case of addition of ten-digit columns, warming-up is approximately zero, fatigue may be measured by the rise from the end of a work period to the beginning of work after rest.

<sup>1</sup> Fatigue, Eng. translation of '04, p. 299.

The advantage of so defining the fatigue effect is that its mixture with practice effect is obviated. If the more usual definition—as the decrease in efficiency caused by the exercise of the function without rest—is used, we are compelled, unless an individual is already at the limit of practice, to analyze (by indirect means) the total change with continuous exercise into a difference between a supposed increased efficiency due to the exercise and a supposed decreased efficiency due to its continuity. The best method of approximately attaining this end is precisely to compare the efficiency at the end of one work period with the efficiency at the beginning of the next after adequate rest (or rest plus warming-up). So it is wiser to define fatigue as that fact from which in any case we infer, and by which we measure, it.

In the case of the five subjects the difference in efficiency between the last ten minutes of 1½ or 2 hours adding and the first ten minutes of the next period, was as shown in Table VI. The central tendency is toward a fatigue effect of 6 per cent. (*i. e.*, to do 6 per cent. fewer examples in the same time) from approximately 100 minutes addition.

TABLE VI

Time (corrected for errors) required to add  $x$  rows at the end of each work period and at the beginning of the following work period.  $x = 6$  for  $D$ , 2 for  $L$ , 4 for  $Mc$ , 3 for  $R$  and 6 for  $S$ . Also the per cent. which the latter is of the former in each case.

	1 <sup>c</sup> . At End of Work Period 1	2 <sup>b</sup> . At Begin- ning of Work Period 2	3 <sup>c</sup> . At End of Work Period 2	3 <sup>b</sup> . At Begin- ning of Work Period 3	3 <sup>c</sup> . At End of Work Period 3	4 <sup>b</sup> . At Begin- ning of Work Period 4	2 <sup>b</sup> /1 <sup>c</sup>	3 <sup>b</sup> /2 <sup>c</sup>	4 <sup>b</sup> /3 <sup>c</sup>	Av. b/Av. c
<i>D</i>	715	565	595	620	615	580	79	104	94	92
<i>L</i>	975	615	590	560	545	585	63	95	107	88
<i>Mc</i>	543	531	579	510	511	466	98	88	91	92
<i>R</i>	630	545	615	545	535	495	87	88	93	89
<i>S</i>	897	850	842	934	779	743	95	111	95	100

Average of all 15 per cents = 93. Median of all 15 per cents = 94.

To those who have accepted certain common notions about mental fatigue this may appear preposterously small and at variance with the results obtained by other workers. This appearance is due in the main to a confusion of the conclusions

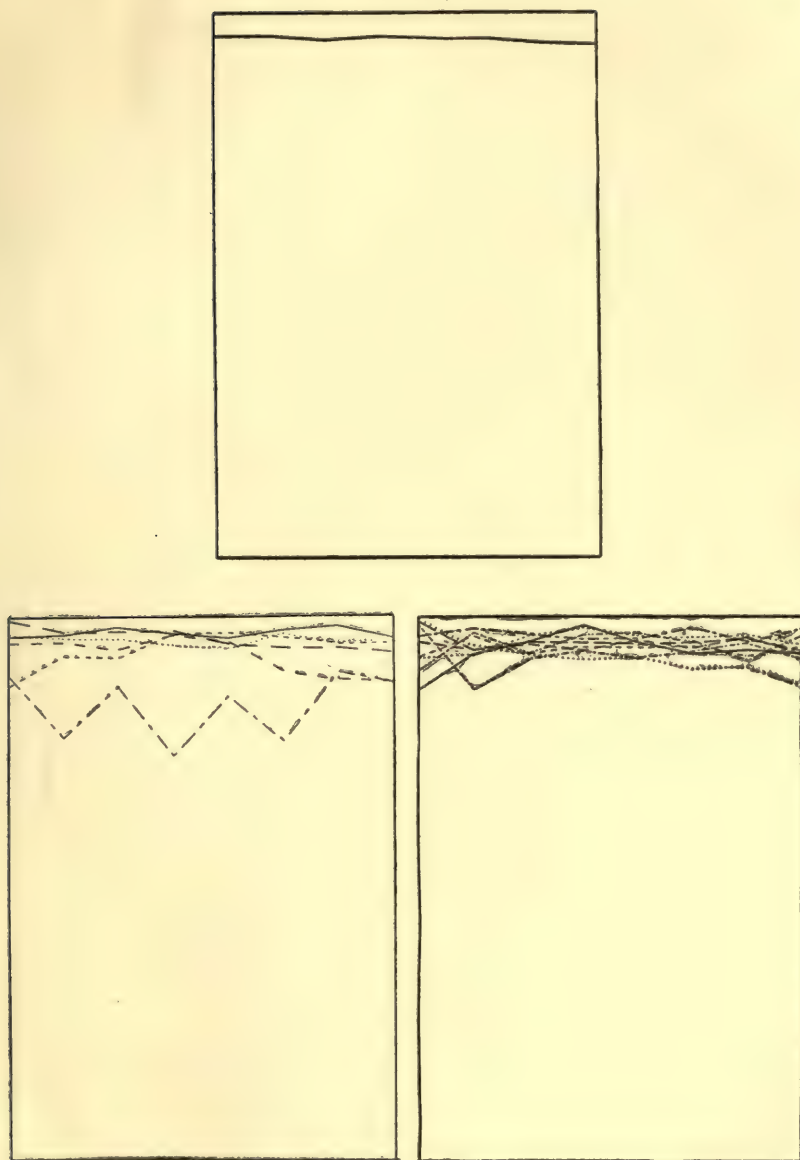
of these other workers with their data. The actual facts concerning the diminution of efficiency in a function as a result of its exercise for two hours without rest show in general an effect so slight as to be almost or quite balanced by the practice effect of the exercise.

Indeed, the most extensive study of Kraepelin's own pupils, that of Oehrn, gives as a general result from 60 two-hour work periods (ten subjects, each with six sorts of work) a curve for the eight successive 15 minute periods as shown in Fig. 8. In general, that is, the subjects worked equally well throughout the entire two hours. This general result might have come from a steady improvement in some of the functions, balanced by a steady loss in efficiency in others, or from various rates of change in efficiency in the different functions. As a matter of fact, however, as Fig. 9 shows, the different particular functions follow closely the general tendency. Their slight divergences therefrom are probably due to the small number of subjects and experiments. Each person, too, might have had a different work-curve, some getting better at points when others were getting worse. As a matter of fact, however, the ten individuals' work-curves follow closely the general work-curve which represents the central tendency of them all. The facts are shown in Fig. 10. Lindley's three subjects gave as a median result for continuous adding for one hour the curve shown in Fig. 11.

In general, in the case of the mental functions so far tested, the subjects maintain about the same efficiency from the beginning to the end of a work period of one or two hours unless the function is one at which they have had little practice. Then they usually *improve* from beginning to end. It is a fact, though one overlooked by many writers on fatigue, that a diminution in efficiency of over ten per cent. from two hours exercise of a function has very rarely been observed directly.<sup>1</sup>

<sup>1</sup> The two clear cases of a large fatigue effect are the results of Weygandt and Bolton. I calculate from Weygandt's data that with his three subjects 24 hours rest increased the number of additions done in 15 minutes by a fourth over the number done in the last 15 minutes of a 90 minute work period. From the four work periods that are available in Bolton's data it appears that 23½ hours rest plus a half hour's practice increased the number of additions by three fifths over the number done in the last 15 minutes of a two hour work period; and that a half hour's rest made an increase of a fourth. Such cases are rare.





FIGS. 8, 9, AND 10. The Results of Oehrns Measurements of the Efficiency of Eight Successive 15-Minute Periods in the Case of Ten Subjects, Each Tested in Six Different Functions. Fig. 8 shows the Central Tendency of All Sixty Work Curves. Fig. 9 Shows the Work Curves in the Cases of the Six Functions Tested. Fig. 10 Shows the Work Curves in the Case of the Ten Subjects.

Such has been inferred, either as a result of false interpretations of the units of measure, or of the supposition that any decrease in two hours continuous work below the practice effect when the two hours are distributed in the best possible manner is to be reckoned as fatigue.

The latter procedure seems plausible, but is essentially unsound. For, example, suppose that I had argued concerning

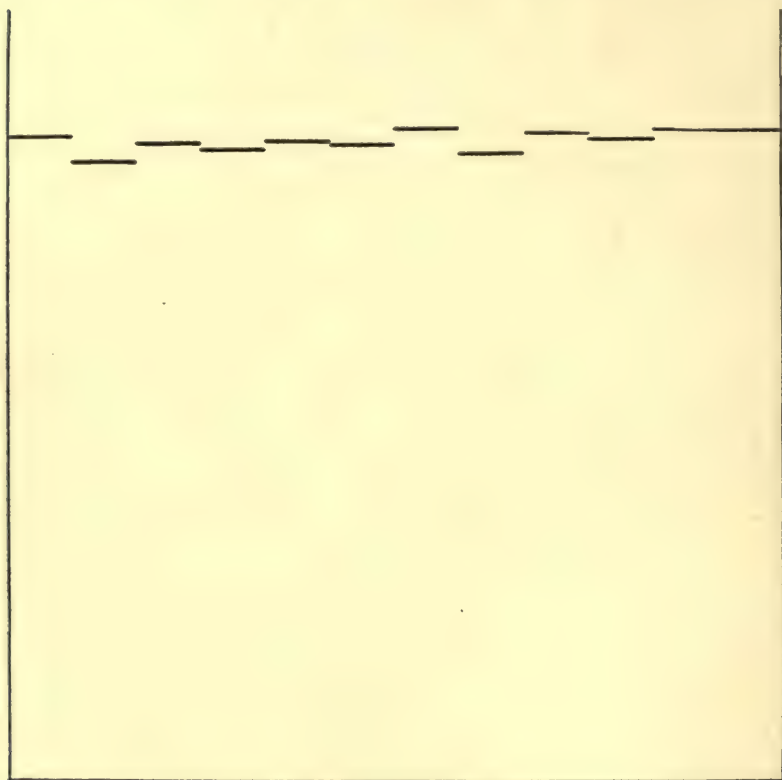


FIG. 11. The Average Results of Lindley's Measurements of the Efficiency of Twelve Successive 5-Minute Periods in the Case of Three Subjects.

my five subjects as follows: An hour of exercise in adding 10-digit examples distributed in seven daily work periods of approximately equal length produces in educated adults an improvement in the time (corrected for errors) of 30 per cent.<sup>1</sup>

<sup>1</sup>This has been found to be substantially correct by Thorndike and by Wells.

Two hours of exercise distributed over 14 days may fairly be expected to produce an improvement of at least 38 per cent. Two hours of continuous practice, however, produced in our five educated adults a change of less than 8 per cent. improvement. Therefore the fatigue due to the two hours work is at least 30 per cent.

Every one of these statements is true except the last. Its falsity is revealed by the following facts: First, on the next day at the beginning of work these subjects did not do 30 per cent. better. They did as a matter of fact 12 per cent. better. Second, if the experiment is continued, and fatigue is again estimated by this method of comparison of actual gain or loss with the gain from equal exercise optimally distributed, the amount of fatigue decreases enormously. No one acquainted with the facts would claim for the last two hours of eight optimally distributed, a practice gain in educated adults of over 13 per cent.<sup>1</sup> It would probably be very, very much less. But in the two continuous hours forming the 7th and 8th in the experiment, the general change was a loss of 1 per cent. The fatiguing effect of this two hours is thus, at the most, 14 per cent., or half of what it was a few days earlier. This discrepancy has to be explained by supposing that the four two-hour periods of exercise have had a three-fold effect: (1) that they have permanently improved ability as much as eight hours optimally distributed exercise would have done, but (2) they have also temporarily so diminished ability as to counter-balance this on each day and (3) they have also enormously diminished their power to temporarily diminish ability. Unless the first and third of these statements are true the second cannot be. The first we have seen to be false in the case of the two hours exercise.

But it must *always* be false unless the optimal distribution of time is that of the work period of the fatigue experiment in question. For one distribution of time,  $D_1$ , to give the same amount of permanent improvement as another,  $D_2$ , can mean only that in later trials under identical conditions those who

<sup>1</sup> In the study of one hour's distributed exercise quoted above, nearly three fourths of the gain was made in the first half of the exercise.



had been subjected to  $D_1$  show the same superiority to their earlier scores as do those who have been subjected to  $D_2$ .

In general, if two random groups,  $A$  and  $B$ , of equal ability at the date  $D_0$  in the function  $M$ , are both given  $X$  hours practice during the time from  $D_0$  to  $D_n$ , and if in a selection of tests after  $D_n$  that are random as to date, length and all other external conditions, groups  $A$  and  $B$  show equal efficiency, then, no matter what difference there was between  $A$  and  $B$  in the way the  $X$  hours were distributed, that difference can have made no difference in the amount of permanent improvement in  $M$ .

The test of the relative practice effect of continuous and broken exercise is not in some mythical changes which would show themselves if fatigue, warming up, additional practice and the like did not interfere and hide them. It is in a fair sampling of some actual operations of the function.

In short, permanent improvement can mean only the gains shown, first, at the end of work plus a length of time such as just provides full rest, and second, at any given selection of later dates. If continuous work does produce the same permanent improvement as optimally distributed work, then the decrease of the score at the end of a two-hour period below that at the beginning of the next is the measure of its smaller temporary improvement. If continuous work does not produce the same permanent improvement as optimally distributed work, the measure of its smaller temporary improvement cannot be the decrease in the gain from  $X$  hours' continuous work below what would have appeared if the  $X$  hours had been optimally distributed.

### PRACTICE

If fatigue is defined as the diminution in efficiency which rest can cure, practice effect may be objectively defined as the increase in efficiency due to a given amount of exercise distributed in a given manner, the conditions of rest and fore-exercise being the same. That is, the practice effect is always a measure of a difference between two tests separated by an interval of time. It is the effect of that interval as well as of

the exercise which fills all or part of it. However practice effect be defined, the fact from which it is in any case inferred is always  $X$  exercise of the function distributed in some way over  $Y$  time. In once for all accepting this complex causation of the practice effect, we really simplify thought.

Kraepelin, in assuming that the practice effect proper should be defined as the increase in efficiency due to the *mere amount* of exercise of the function, has to assume further an effect of the *mere time*, whereby a constant loss of practice effect is taking place. Thus any actual measure of improvement from one test to another is the result of  $GP - LP$ , the gain due to the practice alone less the loss due to the time alone. The inference of the amounts of  $GP$  and  $LP$  permits speculative fancy to run riot. An innocent improvement of 2 per cent. from the first quarter hour's work of an hour on Monday to the first quarter hour's work of an hour on Tuesday may thus become a gain of 10 per cent. from the practice minus a loss of 8 per cent. from 24 hours, or a gain of 20 per cent. minus a loss of 18, or a gain of 200 per cent. minus a loss of 198! We have also the scientific fiction of a function gaining and also losing during the very hour of its exercise—gaining by the exercise and losing by the hour.

The naïve procedure of leaving the 'amount of exercise distributed in a given way' as the unanalyzed cause of the practice effect, which has been customary amongst psychologists other than Kraepelin and his followers, seems therefore preferable.

#### SUMMARY

The essential empirical facts about the curve of mental work seem then to be as follows: Two hours or less of continuous exercise of a function at maximum efficiency, so far as the worker can make it so, produce a temporary negative effect, curable by rest, of not over ten per cent., and in most functions still less than that. The permanent practice effect is much less than that of equal time distributed in fractions over a week or more. Fluctuations of considerable amount occur in any one work period for any one subject, but except for a rise in efficiency of approximately 4 per cent. near the end when the

date of the end is known, no regularity in them has been proved for any one of them for any one subject in any one sort of work, much less for any one subject in all sorts of work, or for all subjects in any one sort of work. The supposed laws that the very first few minutes and the minutes after a drop in efficiency are periods of specially high efficiency are not supported by the facts. A special gradual increase in efficiency in the first fifteen or twenty minutes is not demonstrable in the case of the simple functions such as addition, mental multiplication, marking words of certain sorts and the like. The fluctuations in a single day's record for a single subject are then in no sense explained by referring them to fervor at starting, fervor after disturbance, fervor after fatigue, incitement or adaptation.

The most important fact about the curve of efficiency of a function under two hours or less continuous maximal exercise is that it is, when freed from daily eccentricities, so near a straight line and so near a horizontal line. The work grows much less satisfying or much more unbearable, but not much less effective. The commonest instinctive response to the intolerability of mental work is to stop it altogether. When, as under the conditions of the experiments, this response is not allowed, habit leads us to continue work at our standard of speed and accuracy. Such falling off from this standard as does occur is, in the writer's opinion, due to an unconscious reduction of the intolerability, by intermitting the work or some parts of it.

By this view we have a possible explanation of the decreased practice- or learning-value from continuous rather than distributed exercise. The connections, in the former case, though formed as frequently, would result in far less satisfaction and so in less close bonds. There are, of course, other advantages in distributed exercise, but this difference in satisfyingness would explain the notable puzzle that the permanent effect of work without rest upon learning is greater than its temporary effect upon efficiency.



## SPECULATIVE ANALYSES OF THE CURVE OF WORK

So far in this paper fatigue, warming-up, initial spurt and the rest have been considered as objective features in a work-curve. Psychologists have also often used these terms as names for mysterious forces which caused efficiency to rise and fall. *Antrieb* is thus an inner fervor or impulse which causes a spurt; *Anregung* is thus an inner incitement which causes the warming-up effect. *Fatigue* is thus the inner cause of diminished efficiency rather than the diminution itself.

A work-curve may then be considered as the gross result of a compounding of all these forces in various degrees, and may be analyzed into imaginary curves, each corresponding to the action of one of these forces. The analysis which anyone makes will depend upon his theories about their separate action, and will display those theories clearly.

Let us then, for contrast with the views which I have suggested in this paper, examine Kraepelin's analysis of the following work-curve: In six successive 5-minute periods before a 30-minute rest, and in six immediately thereafter, the observed numbers of additions were: 483, 473, 478, 473, 486, 474, rest, 496, 510, 513, 489, 497, 494.

Kraepelin's explanation of the above case is<sup>1</sup> as follows: The efficiency of the function, apart from practice, fatigue, incitement, adaptation, and fervor from starting, finishing, etc., is 393. The effect of practice is to raise this by 56, 40, 37, 34, and 32 from one to another of the first six periods respectively; of this total practice gain of 199, 149 is lost during the 30 minutes of rest;<sup>2</sup> during the last half hour the practice gains from one to another of successive 5-minute periods are 31, 29, 27, 26, 25. Thus work under the influence of only practice and the 30-minute rest would, in Kraepelin's opinion, give a curve of 393, 449, 489, 526, 560, 592 (rest here), 443, 474, 503, 530, 556, 581 as shown in the upper continuous line of Fig. 12.

The effect of fatigue is to lower efficiency from period to

<sup>1</sup> Kraepelin, E., '02, 'Die Arbeitscurve,' *Philosophische Studien*, Vol. 19, pp. 459-507.

<sup>2</sup> Or strictly from the last 5-minute period before to the first one after the rest.

period successively by 41, 41, 41, 41, 41, a total loss of 205; the rest raises it by 197 (in divisions per 5 minutes of 80, 45, 35, 26, 7, 2, 2); after rest fatigue again lowers it in successive steps by 41, 41, 40, 39, 38. The work under the influence of only fatigue and the rest would, in his opinion, give a curve of 393, 352, 311, 270, 229, 188, (rest here) 385, 344, 303, 263, 224, 186, as shown in the dash line of Fig. 12. Practice, fatigue and the rest together would give the middle curve of Fig. 12.

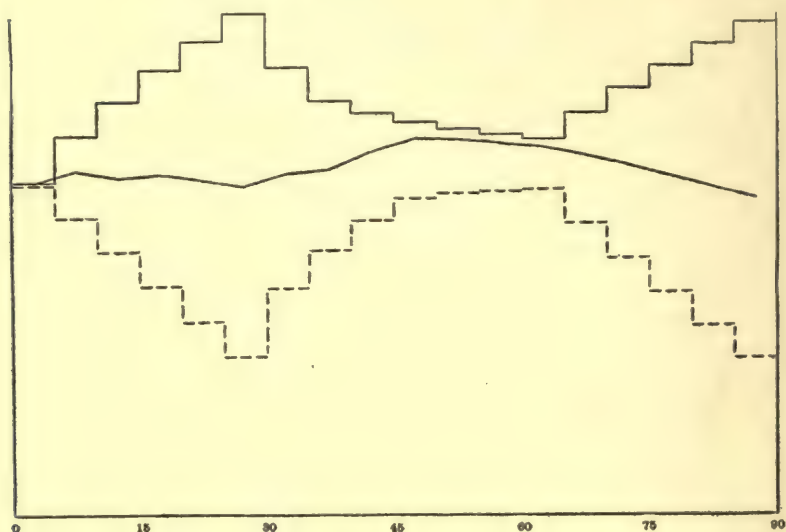


FIG. 12. Kraepelin's Estimates of the Effects of Practice and Fatigue during 30 Minutes Continuous Adding, 30 Minutes Rest, and 30 Minutes Continuous Adding. The Horizontal Scale is of Time; the Vertical Scale is of Number of Additions. The Upper Line gives the Efficiency of the Function by Practice; the Lower Line Gives the Efficiency of the Function by Fatigue; the Middle Line Gives the Combined Result of Both.

Incitement or 'warming-up' is supposed by Kraepelin to raise efficiency from period to period by 35, 5, 5, 0, 0, all the 45 being lost during rest and the gains after rest being 35, 10, 0, 0, 0. The effect of adaptation is supposed to be, from period to period, 30, 5, 5, 5, 5; 5 of this total 50 is lost during rest; after rest the gains due to it are 5, 4, -8, 10, 1. This effect of incitement is shown in Fig. 13 by the line of long dashes; that of adaptation by the line of short dashes.

The effect of fervor at starting is to make the first five-minute period better by 90, and the first one after rest better by 16, than they would otherwise have been.

Minor fluctuations make the third period 4 worse, the fourth, 15 worse, and the sixth, 8 worse, than they would otherwise have been. The ninth and tenth periods are so influenced to the extent of  $-1$  and  $-2$ . Fervor at the approach of the end makes the last period of the whole series better by 18, and the next to the last better by 9, than they would otherwise have been. The effect of these various *Antriebe* is shown by the dot and dash line of Fig. 13.

Incitement, adaptation, beginning and end fervor, and minor fluctuations all together thus would with the rest give a

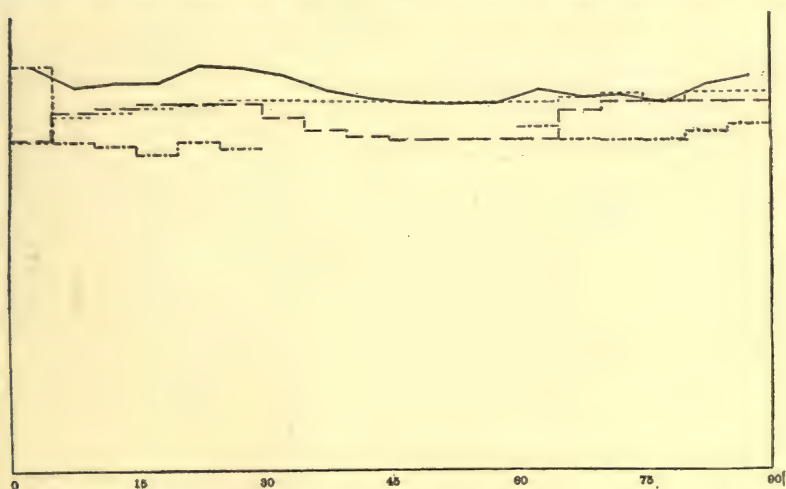


FIG. 13. Kraepelin's Estimates of the Effects of Incitement, Adaptation, Fervor at Starting and Fervor at the Approach of the End during 30 Minutes Continuous Adding, 30 Minutes Rest, and 30 Minutes Continuous Adding. Scaling as in Fig. 12. The Long Dash Line is for Incitement; the Short Dash Line is for Adaptation; the Dash and Dot Line is for Fervor at Starting, Ending, Disturbance, etc.

curve of 483, 448, 464, 463, 483, 480, (rest here) 454, 478, 491, 482, 503, 513, as shown by the continuous line of Fig. 13.

This analysis by Kraepelin is an instructive illustration of the danger of speculative *ex post facto* explanation, for it is certainly highly improbable in almost every one of its main features. If a man of Kraepelin's gifts and training could so



err, others surely can be rarely trusted to imagine the causes of a given curve of work.

The first error in the analysis is the assumption that 30 minute's continuous exercise will add 50 per cent. to efficiency in addition in an educated adult who already adds any one-place to any two-place number in three quarters of a second. No expert in the psychology of practice and of reaction-time would dare to assume so great a gain in so short practice in a function already so far improved.

The second error is the assumption that, in an interval of 30 minutes rest, three fourths of the practice gain is lost. All studies of practice in the case of functions like addition show great permanence of effect after many hours and even days.

The only explanation of their results that would be consistent with any such loss within 30 minutes as Kraepelin here assumes, would be that, even within five minutes or less, there is an enormous decrease in efficiency due to fatigue, so that the net apparent gain from day to day was only a small fraction of what is gained in one day, but lost by fatigue—this in experiments in which the best efforts of the investigator were directed toward avoiding the possibility of any fatigue at all!

A third error is in the assumption that the total practice effect of an hour's addition broken by a 30-minute rest is inferior to that of half an hour's addition alone (plus 188 and plus 199). It might be that in a few individuals no practice effect would come from a second half hour of addition after such rest, but it is enormously unlikely.

A fourth error is the assumption that the fervor of beginning has five times<sup>1</sup> the potency of the fervor from the approaching end of work. There is, on the contrary, a preponderance of evidence to support the belief that, of the two, finishing spurt is more influential than initial spurt in mental work generally and in addition in particular.

A fifth error is the assumption that the fatigue from 30 minutes work will, apart from practice, incitement and adaptation, reduce efficiency by over half. We can reduce the influence of practice, incitement and adaptation by testing indi-

<sup>1</sup> Three times from one point of view.

viduals at or near the limit of practice, and in the second half of a 90-minute period. We do not then find a reduction at all comparable to the assumed 52 or 53 per cent.

A sixth error is the assumption that a half hour's rest will cause seven and a half times as great a percentile loss of the effect of practice as it does of the effect of adaptation! Adaptation, to mean anything distinguishable from practice, should mean an improvement that is *shorter-lived*, that can occur again and again day after day as practice cannot, because it dies out during lack of exercise of the function as practice does not.

The simple fact is that the supposed fatigue of fifty per cent. in thirty minutes and the supposed initial spurt of twenty-five per cent. in two or three minutes are both myths. The enormous practice by a half hour's adding, and the enormous loss of its effects in a half hour's rest, are myths unconsciously invented to support the first myth; the warming-up and the adaptation are modelled into the mythical form necessary to support the second myth.

With increased knowledge and greater watchfulness against inconsistent and improbable suppositions, a psychologist today could improve upon Kraepelin's analysis, but any and all such explanations after the fact are exceedingly risky. In the present state of knowledge it is far better to analyze a work curve by experiment than by deduction. If one thinks that a knowledge of the approaching end produces five per cent. gain in the last ten minutes of an hour, he should have the subject work at the task both with and without knowledge of the approaching end and with all other conditions identical. The difference then found is the effect of knowledge of the approaching end. If certain drops in the curve are thought to be due to disturbances, the disturbances in question should be withheld and applied or increased and decreased experimentally until what difference they actually do make has been measured. The influence of initial spurt, warming up, adaptation, and fatigue upon the early course of the work-curve should be made the subject of speculation only after each of these mythological entities has been defined as some state of affairs in or

out of the organism that can be observed to happen or not to happen, or to happen in varying measurable amounts, and after records have been made of the subject's achievement as related to different amounts and combinations of these states of affairs. At present it is purely fantastic to replace the given fact that in the first twenty minutes such and such a product is achieved, by (a) estimates of what that product would have been apart from initial spurt, that is, what work would have been done in the beginning if there had been no beginning, (b) estimates of what that product would have been apart from 'warming-up' (that is of what work would have been done at a new task if the task had not been new), and (c) estimates of what that product would have been apart from fatigue (that is of what work would have been done if no work had been done)!



# COLORED AFTER-IMAGE AND CONTRAST SENSATIONS FROM STIMULI IN WHICH NO COLOR IS SENSED

BY C. E. FERREE AND GERTRUDE RAND

*Bryn Mawr College*

I. Introduction.....	195
II. Cases in which colored after-images and contrast sensations are aroused by retinal excitations which do not directly condition color sensation....	204
1. After-images.....	204
i. In central vision.....	205
ii. In peripheral vision.....	208
2. Contrast.....	221
3. The Purkinje-Brücke phenomenon.....	224
i. Evidence that it is an after-image of a previous contrast sensation, rather than contrast in the after-image.....	228
ii. But if a contrast effect, evidence that it may take place when the inducing color is unsensed.....	234
III. Explanation.....	236

## I. INTRODUCTION

In the March number of the *PSYCHOLOGICAL REVIEW*, 1907, Thompson and Gordon<sup>1</sup> describe a series of experiments in which colored after-images are obtained in the peripheral retina from stimuli in which no color was sensed. In the November number, 1905, and in the January number, 1908, of the same journal, Fernald<sup>2</sup> working under approximately the same condition reports similar results. In the *Proceedings of the American Philosophical Society*, 1908, however, Titchener and Pyle<sup>3</sup> deny the phenomenon, affirming their complete inability to get colored after-images when no color is sensed in the stimulus.

<sup>1</sup> Thompson, H. B., and Gordon, K., 'A Study of After-images on the Peripheral Retina,' *PSYCHOL. REV.*, 1907, XIV., pp. 122-167.

<sup>2</sup> Fernald, G. M., 'The Effect of the Brightness of Background on the Extent of the Color Fields and on the Color Tone in Peripheral Vision,' *PSYCHOL. REV.*, 1905, XII., p. 405; 'The Effect of the Brightness of Background on the Appearance of Color Stimuli in Peripheral Vision,' *PSYCHOL. REV.*, 1908, XV., pp. 33-35.

<sup>3</sup> Titchener, E. B., and Pyle, W. H., 'On the After-images of Subliminally Colored Stimuli,' *Proc. of the Amer. Philos. Soc.*, 1908, XLVII., No. 189, pp. 366-384.

With regard to these discrepant reports, nothing more will be attempted at this place than to point out clearly the position held by each investigator. Titchener and Pyle contend that under no condition known to them can colored after-images be obtained from stimuli in which no color is sensed. Thompson and Gordon, and Fernald claim that under the conditions described by them, after-images may be obtained in which the color is clearly distinguished. The point at issue between them, then, is not whether the phenomenon can be gotten under this or that condition usually obtaining in the work on after-images, or under any given percentage of possible conditions, but whether it may be gotten by any experimental device whatsoever. Nor is it meant to extend the phenomenon to brightness sensation.<sup>1</sup> So far as the writers know, no one has ever claimed to be able to get an after-image from a brightness stimulus too weak to arouse sensation. The question, therefore, whether a subliminal sensory excitation can produce a supraliminal after-effect, considered in a general sense, is in no fashion under discussion. With the issue thus stated, the writers are impelled to take the affirmative side of the question by the experimental results they have obtained, and scarcely less strongly by theoretical considerations of the difference in the effect of different members of the brightness series upon all the colors when fused with them, and in the effect of a given brightness upon different colors. Working under the right conditions, the phenomenon is easily obtained. That its explanation is not essentially difficult will be shown in a later section of this paper.

The work on the peripheral retina has been repeated by the writers and the investigation extended to include other cases in which colored after-image and contrast sensations may be aroused by stimuli in which no color is sensed. It is the purpose of this report to describe these experiments and to explain the results obtained in terms of visual phenomena already known.

Before passing to a description of our own experiments it may be helpful to examine the work of previous investigators in

<sup>1</sup> For the sake of brevity, brightness is used here as a general term for the colorless sensation series, white, black and the grays.

order to determine from neutral ground if possible the cause of the disagreement in the results they have obtained.<sup>1</sup> In general two types of method have been used in arousing the color excitation. In one the color was kept below the limen of sensation by adaptation; in the other it was obscured by the action of an unfavorable brightness excitation. The former method was first used by Tschermak.<sup>2</sup> Tschermak's article is not a description of experimental work. It is an essay in which the author in part seeks to trace the analogies to visual adaptation found in the reactions of other tissue: muscle, nerve, secretory, etc., to external stimuli. The reference to the phenomenon we are discussing is very brief in this article and the description of the conditions under which it was obtained is quite inadequate to serve as a guide for future work. He says: "Haben wir doch gerade in der Anwendung des constanten Stromes auf Nerv und Muskel ein vorzügliches didaktisches Mittel, um die Grundbegriffe der allgemeinen Reiz- und Adaptationslehre zu veranschaulichen und einzuprägen. Am besten demonstrieren wir als Gegenstück zugleich die Wirkung eines mässig satten Farbglasses auf das Auge: die Phase der Reizwirkung, individuell verschieden lang, und dadurch erinnernd an die verschiedenrasche Adaptation des Präparates vom Warmfrosch und Kaltfrosch an den constanten Strom—weiterhin das Stadium der vollendeten Adaptation, endlich den gegensinnigen Oeffnungseffect. Nicht minder lehrreich ist die Parallele des subjectiven und des objectiven Erscheinungsgebietes für das Phänomen des Einschleichens d. h. des Ausbleibens einer sinnfälligen Reizwirkung, wenn der Reiz so langsam anwächst, dass das Adaptationsvermögen folgen kann—gleichwohl hat auch nunmehr Wegfall des 'Reizes' eine gegensinnige Oeffnungswirkung. Analoges gilt vom Ausschleichen, also vom Ausbleiben eines sinnfälligen Oeffnungseffectes. Zum optischen Versuche schiebt man zweckmässig successive eine schwach tingierte Glasplatte vor die andere oder benützt einen Keil farbigen Glases."

<sup>1</sup> Because of the inadequacy of Tschermak's description of his method of working this can not be attempted for the work in the central retina.

<sup>2</sup> Tschermak, A., 'Das Anspannungsproblem in der Physiologie der Gegenwart,' *Archives des Sciences biologiques*, Sup. Band, 1904, XI., pp. 79-97.



In 1901 Titchener and Pyle carefully repeated and elaborated upon Tschermak's experiment on the central retina with entirely negative results. The writers can only commend the thoroughness with which they seem to have done this work. Their work on the peripheral retina is introduced with the following words. "We have already mentioned the experiments made by Titchener in 1906 with the view of testing the conclusions of Miss Fernald's first paper. The observations were rigorously confined to the black-white zone, and the outcome was definitely negative. In the meantime, however, the arousal of a colored after-image by a subliminally colored stimulus had been maintained for both the blue-yellow and the red-green zones. Unsystematic observations made in the Cornell Laboratory failed to confirm this result. It seemed worth while, however, to obtain further testimony; and Professor Baird, of the University of Illinois, very kindly consented to investigate the subject. The experiments were carried out by means of a simplified form of the Zimmermann perimeter, which permitted an accurate record of the degree of eccentricity at which the stimulus was exposed. Exploration was confined to the horizontal nasal meridian of each eye. The stimulus was a beam of light from an electric (16 c.p.) lamp, transmitted through appropriate combinations of gelatines and colored glasses; the colors employed were (non-equated) blue and yellow, red and green. Six of the most reliable laboratory students acted as observers, and Professor Baird had personal charge of the entire work. The after-images were projected upon white, gray and black grounds. The experiments proper were preceded by a careful determination of the outermost limits of color vision for the stimuli used, and all pains were taken to avoid chromatic adaptation" (pp. 376-377).

Professor Baird reports negative results in every instance. With regard to this work the writers can not help but observe that Baird has failed to conform to the conditions which Fernald had said are essential for getting the phenomenon. Without drawing upon their own experiments for a knowledge of essential conditions, they will point out three conditions which Baird has apparently failed to fulfill, the neglect of any

one of which is amply sufficient to account for his results being negative. (1) Fernald lays great stress upon the use of a campimeter screen by means of the induction from which the brightness conditions were obtained which obscured the color in her stimulus.<sup>1</sup> Baird used a simplified form of perimeter, how simplified Titchener and Pyle do not state. (2) Baird uses for the duration of the stimulation intervals of 30 to 40 seconds. Fernald is careful to state that the interval of stimulation should not exceed three seconds.<sup>2</sup> (3) In her description of conditions Fernald states that the color should be exposed behind the opening in a campimeter screen, and the card upon which the after-image is projected should be slipped in between the colored surface and the stimulus opening. Thereby the campimeter screen and thus the larger part of the field of vision remains unmoved and the least possible incentive is given for involuntary eye-movement. With Baird's apparatus, however, we would judge that the ground upon which the after-image was projected must have been moved in between the stimulus and the observer's eye, thus exerting a strong incentive to drag the fixation with it. A very slight eye-movement indeed is amply sufficient to blot out or to prevent from developing the instable peripheral after-image.

<sup>1</sup> While, as will be shown later in the article, the writers do not hold as Miss Fernald does that the inductive action of the campimeter screen is an essential or even a favorable condition, still they do insist that a campimeter screen or its equivalent is necessary in order to be able to give a projection field for the after-image without causing an amount of involuntary eye-movement that would prevent the momentary and instable peripheral after-image from developing.

<sup>2</sup> With regard to the importance of this point the writers are in entire agreement with Fernald. In fact one does not need to work long with peripheral after-images to be convinced that so long an interval as Baird used is absolutely prohibitive of colored after-images even when a less excentric portion of the retina is investigated than was the case here. A short interval of stimulation is necessary because of the rapid adaptation of the peripheral retina to color. It is well known that adaptation to color in any part of the retina takes place rapidly at first and then progressively more slowly until a stationary point is reached. Working in the central retina the writers have found a similar curve of effectiveness for after-images. After-images seem to occur most intensively when the stimulus is removed while adaptation is still going on. If one carries the stimulus beyond a stationary point in adaptation, the after-image will weaken roughly in proportion to the length of time during which the stimulus is regarded after the stationary point has been reached. This is true with both intensive and slightly supraliminal stimuli.

Failing to obtain the after-image in the central retina and to get confirmation by Baird that it may be obtained in the peripheral retina, Titchener and Pyle suggest explanations for the positive results gotten by Tschermak and Fernald. "The outcome of Tschermak's observations with the glass wedge must then in our opinion be explained by the prepossession of the observer and the roughness of the method employed. . . . It is less easy to account for the peripheral results." For a full statement of their explanation of the peripheral results the reader is referred to their article, pp. 378 ff. Brief mention only can be made of it here. We wish to comment on three points. (1) Their conception of the problem of getting color in the after-image when none is sensed in the stimulus is, we believe, different from that held and stated by Fernald. "The *experimentum crucis*," they say, "would be the production of a colored after-image in the achromatically adapted eye at a point lying well beyond the limits of blue-yellow vision." It is conceivable that two interpretations might be given to "well beyond the limits of blue-yellow vision." (a) The meaning might be well beyond the limits of blue-yellow vision whatever area and intensity of stimulus be used. And (b) it might be well beyond the limits at which these colors are seen when the stimuli used in the after-image experiments are employed. Wishing in every case to favor the point criticised, we choose the second interpretation. Thus to obtain a colored after-image well beyond the limits of blue-yellow vision would imply that a negative color excitation sufficiently strong to arouse sensation could occur in response to a stimulus which can under no condition arouse a positive color sensation. This is not at all the claim of Fernald nor of Thompson and Gordon as we understand their claim. They believe, in the cases cited by them, that the stimulus was prevented from arousing positive sensation by unfavorable brightness conditions. While it may not be clear from their work how these unfavorable brightness conditions prevent the positive sensation and permit the negative, there can hardly be any doubt that they would not claim that colored after-images can be aroused in the retina at a point "well beyond the limits of color vision." In fact in her



paper of 1905 Fernald specifically states that one should work just within the limits of color vision.<sup>1</sup>

(2) It is pointed out by Titchener and Pyle that the limits obtained by Miss Fernald show a considerable range of variability. The "*experimentum crucis*" thus has been inadequately performed. She has in reality obtained the after-image within the limits of blue-yellow vision. As pointed out in (1), this, we believe, is not the point at issue. The question is not whether the after-image can be obtained beyond the limits of color vision, but whether it can be obtained anywhere in the retina when color is not sensed in the stimulus in a percentage of cases large enough to preclude the possibility of its being due to chance happening, to error of observation, or what not. Apparently neither Fernald nor Thompson and Gordon, and certainly not Tschermak, have entertained the idea that the colored after-image can be obtained at any point on the retina where experiment shows that color can not be obtained in the positive sensation for the given stimulus under any conditions whatever.

(3) Titchener and Pyle quote the following observations furnished by Miss Fernald by private correspondence.<sup>2</sup>

<sup>1</sup> In her paper of 1908 Fernald states that the after-images are perceived most frequently just inside or just beyond the regular limits for the color. As compared with her first statement, this may seem somewhat loose and might lead to misunderstanding. There can be little doubt, however, that beyond the regular limits means for her beyond the limits obtaining for some given set of brightness conditions taken as standard—not beyond the limits for all brightness conditions. Fernald, it will be remembered, worked with very little control of the factors extraneous to the source of light that influence the sensitivity of the retina to color. The boundaries of her color zones thus varied within quite wide limits. In this region through which the boundaries of the zones varied, she was able to obtain color in the after-image when none was sensed in the stimulus. Her 'regular' limits fell somewhere within this region. She was thus able to obtain the after-image sometimes just within, sometimes just beyond the regular limits. She apparently never obtained the after-image at a point beyond her widest limits of sensitivity to a given color. The present writers, working with a control of brightness conditions that enabled them to duplicate their limits from sitting to sitting within a degree of variation, never obtained the after-image beyond the limits determined under their most favorable brightness conditions.

<sup>2</sup> A request was sent to one of the present writers (Ferree) to determine whether these after-images could be gotten, at the same time the similar request was sent to Dr. Baird. The results quoted are samples of the observations made at that time.

Observer: C. E. Ferree. Full illumination on bright day (May 17, 1908). Nasal meridian, right. White ground. Projection field white, except in obs. 14-17, when it was black. Stimulus, 13 sq. mm. Distance from eye to stimulus, 25 cm.

Fixation Point	Stimulus	Color Seen	After-image
80°	<i>O</i>	Dark gray	Unsaturated light blue
85°	<i>B</i>	Just dark	Wash of unsaturated yellow
85°	<i>Y</i>	Nothing	Nothing
80°	<i>Y</i>	Tinge of dirty yellow	Very pale blue
80°	Medium gray	Dark	White
80°	<i>O</i>	Indefinite gray	Nothing
80°	Light gray	Dark	White
75°	<i>Y</i>	Reddish yellow	Good blue
75°	<i>B</i>	Good blue	Good yellow
75°	<i>B</i>	Good blue	Good yellow
65°	<i>O</i>	Yellowish red	Unsaturated blue
65°	<i>Y</i>	Reddish yellow	Blue
60°	<i>G</i>	Indefinite greenish gray	Uncertain
65°	<i>G</i>	Greenish yellow	Dark red, more saturated than stimulus
80°	Medium gray	Dark	Nothing
80°	Medium gray	Dark	Nothing
65°	<i>G</i>	No color	Flash of red
65°	<i>R</i>	No color	Blue

Commenting on these observations Titchener and Pyle say: "Positive results occur in the first two and last two observations of the series. The former may be explained in terms of chromatic adaptation. If as the illumination suggests the observer began the work in yellow adaptation, the first blue after-image would naturally follow." If the above explanation were adequate, the blue after-image should have been obtained also when gray was used as stimulus. As a check experiment gray was repeatedly used as stimulus by Fernald, and in no case was the characteristic blue after-image obtained.<sup>1</sup> Furthermore the same yellow light which in terms of the explanation gave the stimulation for the blue after-image fell with undiminished intensity on the projection field, hence there was no shutting off and, since the projection field was white, even no diminution

<sup>1</sup> In a footnote p. 382 we find: "These observations were taken after the limits had been roughly determined in previous experiments. If the determination of limits was made at the same sitting, and if the last test color employed was orange, there would be additional reason for an initial yellow-adaptation." In reply to this we would say that in case of our own experiments orange and yellow were purposely not the last colors used in determining the limits; that had they been, abundance of time was given for the complete recovery of the eye before the after-image experiments were performed; and that if chromatic adaptation had been present, its effect should have shown in the after-image when gray was used as stimulus.

in the amount of the yellow daylight reflected to the eye for the period during which the after-image was observed. Thus no chance was given for a blue after-image to develop as the result of stimulation by the yellow in the daylight. Continuing Titchener and Pyle say: "If the second observation was taken at too short an interval of time the resulting blue-adaptation should show itself as a yellow after-image." We interpret this statement with some hesitation. It seems to mean that the first observation gave a blue after-image, and that if the second observation followed too closely upon the first, this after-image excitation in turn aroused a negative excitation which formed the physiological basis of the yellow after-image observed. If this interpretation is correct two conclusions would logically follow. (1) A negative after-image can itself give a negative after-image,—a phenomenon which, so far as the writers know, has never yet been observed. And (2) although a colored stimulus too weak to arouse positive sensation can not arouse a colored after-image, still the excess of yellow in clear daylight, which, reflected from the yellow stimulus, is too weak to be sensed as color, can arouse an after-image which not only can be sensed as color but which in turn can arouse a second after-image in which color can be sensed. "The two final observations suggest a shift of conditions. Green is seen at  $65^{\circ}$  as greenish yellow and at  $60^{\circ}$  as indefinite greenish gray. It is possible that in the case in which 'no color' is reported the green simply escaped notice; peripheral colors at the limit of vision often appear as momentary flashes. Again red is reported at  $65^{\circ}$  as 'no color' although reddish yellow had been seen as far out as  $75^{\circ}$ . It is possible that the flash of red escaped notice; it is also possible that red-adaptation from the preceding after-image brought out the blue." With regard to the possibility of the red and green escaping notice, the following points may be noted. (1) The stimulus color in this region of the retina has not so much a momentary character as the after-image color and would, therefore, not be so likely to escape notice as the after-image color. (2) The observations quoted above were made by one of the writers (Ferree), who here positively asserts that to the best of his knowledge this



was not the case. (3) In our own experiments to be described later in this article, conditions were obtained under which the after-images of red and green were obtained in practically 100 per cent. of the observations made. It seems scarcely possible that the color should have escaped notice in the stimulus in all of these cases and have been observed in the after-image. In case it be held that "red-adaptation from the preceding after-image brought out the blue," we are again asked to accept the thesis that an after-image excitation may in turn give rise to an after-image excitation strong enough to be sensed as color.

## II. CASES IN WHICH AFTER-IMAGES AND CONTRAST SENSATIONS ARE AROUSED BY RETINAL EXCITATIONS WHICH DO NOT DIRECTLY CONDITION SENSATION

### 1. *After-Images*

The problem presented is: Can the color in a stimulus be obscured directly for sensation, and still set up an excitation upon the retina which will give an after-image? Our answer is that it can be done both in the central and the peripheral retina, but possibly more readily in the latter than in the former. To accomplish it, an experimental condition must be devised which will work against the color in the stimulus and relatively favor it in the after-image. A working principle is found in the difference in effect of brightness changes upon the saturation of the colors.

This effect may be expressed as follows. (1) Brightness fused with color inhibits the color sensation. With the exception of the region just within the limit of sensitivity for two colors,<sup>1</sup> the following may be stated roughly as the law of this action for all colors for all parts of the retina. White inhibits

<sup>1</sup> Over a region 3° in width just within the limits of sensitivity as determined with the Hering pigment papers at full illumination, red and yellow have a higher limen in black than in white. In this zone red and yellow darken as compared with their brightness values in the central and paracentral retina. Added to the black of the disc, then, we have the black due to the darkening of the color. Thus the figures which are read from the disc do not express the actual amount of black added to the color. Whether or not we have an exception here to the law of the action of brightness upon color which obtains in the rest of the retina is thus open to question. At least, the exception as expressed by the measurements is exaggerated.

most, the grays in the order from light to dark next, and black the least. (2) A given brightness change does not affect all colors to the same extent. If local and towards either black or white, blue and green lose their saturation completely before red, orange, and yellow; when the change is general and toward black, produced by decrease of illumination, yellow, red, and dark orange are obscured before green and blue. All of these changes were utilized at different times in our experiments both on the central and peripheral retina. It is readily seen that the technique of getting a colored after-image from a stimulus in which no color is sensed becomes merely a matter of fusing the least favorable brightness quality with the stimulus color and the most favorable with the after-image color. When this technique was carried out in its best form, the colored after-image was obtained in practically every case.

### *i. In Central Vision*

Two methods of working were used in these experiments, one in which the brightness control came through changes in the general illumination of the field of vision, and the other in which the changes were local. The former method will be described first.

After-images were obtained of red, yellow, and orange stimuli of the Hering series of colored papers. The work was done in a long narrow dark-room with a small window at one end near the center, darkened by a solid and carefully padded door. By opening and closing this door, the general illumination of the room could be varied at will. The observer was seated in front of the window and to the left, so that the light coming in from above and behind fell free from shadow upon a screen placed in front of him.

The intensity of the stimulus was decreased by decreasing the illumination. Two methods were used in doing this, differing in the amount of dark-adaptation the eye had experienced before the stimulus was given. In the first, a preliminary determination was made of the illumination at which the stimulus was sensed as colorless. The experiment was then conducted at that illumination. The stimulus was exposed

from 5 to 15 seconds, varying with the subject, and the after-image observed. In the second method, the experiment was started at full illumination. The door was opened and the stimulus covered with a card of the same gray as the background. The door was then closed rather quickly until the point was reached at which the color is unperceived, this point having previously been carefully determined. The stimulus was exposed before the eye had become adapted to the decreased illumination, and the after-image was observed. The advantage of the second method is that with it the eye loses its sensitivity to color at a more intensive illumination than when the first method is used. That is, the eye gains in sensitivity to color at any point in a series of decreasing illuminations if it is allowed to adapt to that illumination before the exposure is made. Thus, by taking advantage of the lessened sensitivity of the eye to color with decreased illumination before adaptation sets in, we were able to work at an illumination that gave a stronger objective stimulus, and was more favorable for the development of the after-image. Either method, however, gave us unmistakable color in the after-effect. In order that we might be sure that the observer worked with stimuli in which the color could not be sensed and that color really was distinguished in the after-image, he was in the first place kept in ignorance of the color that was to be used in a given test, and in the second place was given at intervals in the series a gray for stimulus which could not be distinguished from the color at that illumination. Thus having run blank tests, and having used every precaution known to us to eliminate the influence of expectation, etc., we feel a reasonable degree of confidence in the results obtained.

The after-images gotten in this way in central vision are not so saturated as those obtained in peripheral vision, but in proportion to their saturation last much longer than peripheral after-images. The after-image of red shows greatest saturation, dark orange next, and yellow least. As one would expect from its quicker rate of adaptation and from the fact that after-images decrease in intensity after the stationary-point in adaptation is reached, the time of stimulation most favorable for



producing the after-image was shorter for the red stimulus than for the other colors. The time for orange was next shortest, and for yellow the longest.

Local changes in the brightness of color may be produced by objective mixing, by contrast, and by after-image. Of these three methods the addition of brightness as after-image is by far the most successful. By objective mixing the amount of colored light coming to the eye is reduced in proportion to the amount of brightness added. The color thus loses saturation from two causes: the reduction of the amount of colored light coming to the eye, and the inhibitive action of the brightness excitation upon the color excitation. For our purpose, it is of advantage to obtain the loss in saturation without any reduction of the amount of colored light coming to the eye, *i. e.*, by means of the inhibitive action of the brightness excitation alone.<sup>1</sup> This can be done by the addition of the brightness either as contrast or as after-image. Of these two, the after-image method yields the better results, as much more brightness can be added as after-image than can be induced as contrast. This is especially true of the central retina. The amount of inhibitive action that can be obtained by this means may be shown as follows. The limen of Hering red in a gray of its own brightness is for one observer 27°. When the after-image obtained by a stimulation of 10 seconds to black is projected on this color, the limen is raised to 60°. This after-image decreases in intensity very little for the first 8 to 10 seconds, and disappears after from 12 to 15 seconds. 10 to 12 seconds stimulation to color is quite sufficient to arouse an intensive colored after-image. Hence by a preëxposure of 10 seconds to black and by the projection of the after-image upon a colored stimulus, it is possible to keep the color in a fairly intensive colored stimulus below the limen long enough to arouse the excitation necessary to give a colored after-image of considerable intensity.

The method of working is as follows. A small colored disc composed of sectors of the color to be used and a gray of the brightness of this color, is set up on a color-mixer. A second

<sup>1</sup> As will be shown in a later paper, this action inhibits the effect of the color excitation, although it does not reduce its power to arouse after-image and contrast.

disc of black of the same dimensions is set up beside it. As much color is put in the first disc as will be rendered just subliminal by the after-image obtained by a 10 second stimulation to the black disc. The colored after-image is then projected on a black field. The addition of white to the stimulus works against its color, and the black projection field favors the color of the after-image. Proceeding by this method, a colored after-image of good saturation and duration can be obtained in every case. The method can be made still more effective by adding the brightness to the stimulus color by both contrast and after-image. For example, the colored disc may be exposed through an opening in a black screen. The white induced by this screen would then add to the effect of the white after-image and in consequence still more color may be used as stimulus without arousing a color sensation.

## ii. In Peripheral Vision

The color in the stimulus may be obscured in three ways. (a) It may be carried to the peripheral retina near the limit of sensitivity for the color used and a brightness be induced across it that works against its saturation. (b) The unfavorable brightness may be mixed with it as after-image aroused by preëxposure to a brightness stimulus. (c) The stimulus may be carried to some angle of indirect vision not too remote from the fovea and the general illumination be decreased until the color is lost. In (a) and (b) the brightness is mixed with the color by contrast and after-image rather than added to it on the color-wheel or by some other means of objective mixing, because, as stated before, objective mixing decreases the physical intensity of the stimulus. This would decrease the energy of the positive color excitation, which would in turn decrease the energy of the negative excitation, and thus defeat the purpose of the experiment. But the addition of the brightness as contrast or after-image does not affect the energy of the stimulus; and while it reduces the effect of the positive excitation upon sensation, it apparently does not decrease its energy as retinal excitation, for it does not lessen its power to arouse after-images. This strongly indicates that the action of bright-

ness upon color takes place at some physiological level posterior to the seat of the positive and negative color processes, as will be shown in the next paper of this series.<sup>1</sup> Consider method (*b*) for example. Here the brightness is added as after-image. Its effect is to blot out the weakly supraliminal color in the stimulus, but it does not prevent the complementary color from appearing in the after-image. This is readily explained in terms of the above hypothesis as to the level at which this action takes place. If, as we suppose, the inhibition takes place posterior to the level of the positive excitation, the negative excitation is not weakened thereby. And since the brightness after-image which was added cannot itself leave behind a brightness after-excitation, nothing is carried over to the negative color excitation to weaken its effect upon sensation. Hence by this method of working we should get as much color in the after-image as if no brightness quality had been added to the colored stimulus.

While method (*a*) can also be used to obscure the stimulus color, still it is not so effective as (*b*) for obtaining the colored after-image; because (1) by means of it the zone of sensitivity cannot be narrowed nearly so much as by (*b*), and (2) the inducing field throws by contrast the same brightness quality across the after-image as it does across the stimulus, and so, roughly speaking, it inhibits the after-image as much as it inhibits the stimulus.<sup>2</sup> In fact, in the way it has to be used in the peripheral retina method, (*a*) alone is of little use in getting our phenomenon. To make this method work successfully, some means would have to be devised for changing the quality of the inducing surface in the interval between the end of the stimulation and the beginning of the after-image. That is, the stimulation would have to be given on a field which induced a brightness quality unfavorable to the saturation of its color,

<sup>1</sup> See 'An Experimental Study of the Fusion of Colored and Colorless Light Sensations: The Physiological Level at which this Action Takes Place.' (In press.) See also abstract in *Journ. of Phil., Psych., and Scientific Methods*, 1911, VIII., p. 294.

<sup>2</sup> The second objection to this method does not apply to the use of the contrast method in the central retina (see p. 207), for in the central retina the colored after-image is sufficiently stable and durable so that an entirely new projection can be substituted for the screen which induces the contrast. In this case the effect of the induction of this screen can be eliminated from the after-image.



and the after-image observed on a field whose inductive action was favorable to the saturation of its color. This change of the inducing field, however, can not be made for two reasons. (1) The duration of the peripheral after-image is very short. It comes as a momentary flash of color immediately after the stimulus light is shut off, and disappears before a change in the inducing field can be made. And (2) the after-image would be completely extinguished by the eye movement set up by shifting so large a part of the field of vision.<sup>1</sup>

The investigation of favorable conditions was conducted in part by means of the vertical campimeter and in part by means of the rotary campimeter.<sup>2</sup> The campimeter provides three possibilities for the variation of brightness conditions which was needed in our problem. By means of it, the brightness of the local preëxposure, the brightness of the inducing field, and the brightness of the field on which the after-image is projected may be changed at will. The brightness of the local preëxposure and of the field on which the after-image is projected may be varied by changing the cards held in the frame behind the stimulus opening in the campimeter screen. The brightness of the inducing field may be varied by changing the paper covering the campimeter screen. All of these variations were used in our investigation. Fernald recognized the influence of only two of these variations: the campimeter screen and the projection field.<sup>3</sup> And of the two the importance of campimeter screen was very much overestimated, while the impor-

<sup>1</sup> See Ferree, C. E., 'The Fluctuation and Duration of the Negative After-image,' *Amer. Jour. of Psychol.*, 1908, XIX., pp. 68, 87-97.

<sup>2</sup> For a description of the vertical campimeter see Fernald, G. M., 'The Effect of the Brightness of Background on the Appearance of Color Stimuli in Peripheral Vision,' *Psychol. Rev.*, 1908, XV., pp. 27-29. For a description of the rotary campimeter see Ferree, C. E., 'A Description of a Rotary Campimeter,' *Amer. Jour. of Psychol.*, 1912, XXIII. (In press.)

<sup>3</sup> The influence of preëxposure under the ordinary conditions of working was apparently not at all realized by her. Her preëxposure and projection field were in most cases made of the same brightness as the campimeter screen. No reason is assigned for doing this. Some indication of her reason may perhaps be had from Thompson and Gordon who also use a preëxposure and projection field of the same brightness as the campimeter screen. They say: "When the stimulation had lasted the desired time, the screen was again put over the color, thus making with the campimeter a uniform gray surface upon which the after-image could be observed" (*Psychol. Rev.*, 1907, XIV., p. 124).

tance of projection field was relatively underestimated. Moreover she seemed to entertain no clear notion of just how these factors produce their results. For example, the brightness induced by the campimeter screen was supposed to contribute in some fashion to the production of her phenomenon by working against the color in the stimulus and favoring it in the after-image; but just how it operates to do this was left in question. Apparently she had made no quantitative study of the fusion of brightness with color and of the relative influence of the different brightness qualities upon the saturation of color. From the side of explanation, then, she can scarcely be regarded as having a definite point from which to start.

Since the writers report the after-image in so much greater percentage of cases than Miss Fernald, and differ from her so much in their explanations of results, it may be well to discuss her work at this point in greater detail.<sup>1</sup> They have the following comments to make on her general technique and her explanation of how it produced the results she obtained.

1. No systematic attempt was made to determine the factors influencing her phenomenon. Of the full list of achromatic factors, for example: brightness of the surrounding field, brightness of the preëxposure,<sup>2</sup> brightness of the projection field, and the degree of general illumination, she recognizes only brightness of the surrounding field, brightness of the projection field, and the degree of general illumination. Especial stress is laid on the brightness of the surrounding field. As we have already shown, the influence of brightness of surrounding field is comparatively insignificant because it is exerted both upon the color of the stimulus and the color of the after-image. Thus if it is unfavorable to the one it will also be unfavorable to the other. It has a margin of influence, however, due to the fact that it does not act with equal strength upon the stimulus and the after-image. Its action is unequal because stimulus and after-image differ both in color and brightness. There would be on this account a small difference in the amount of induction by the screen in the two cases, due to the difference between the brightness relation of stimulus to background and that of after-image to background; and further a probable difference in the

<sup>1</sup> Fernald's work rather than Thompson and Gordon's is singled out at this point because Fernald has made a more extensive investigation of the brightness factors and their relation to the phenomenon in question than have Thompson and Gordon.

<sup>2</sup> Miss Fernald always used some kind of colorless preëxposure, but apparently she did not recognize its importance as a brightness factor. For her the preëxposure card served in the main merely as a neutral covering for the colored stimulus until a steady fixation could be obtained and the observer be otherwise made ready for the exposure to color. Towards the end of the work reported in 1909, however (p. 29), she apparently began to realize that if the fixation were held for a period varying from three to ten seconds, the preëxposure card would give a brightness after-image which would mix with the stimulus color and lighten or darken it, as the case might be. But under the ordinary conditions of working, she took no account of it as a brightness factor.



inhibitive action of this induced brightness upon the color, due to the fact that the stimulus and after-image are different colors. The factors of predominant importance are the brightness of the preëxposure and of the projection field. These are of greater importance (a) because more brightness influence can be exerted by means of them; and (b) because their action is more differential, *i. e.*, the brightness of the preëxposure acts upon the stimulus alone, and the brightness of the projection field acts upon the after-image alone. The brightness of the preëxposure can thus be made to work against the color in the stimulus and to have no effect upon the color in the after-image, and the brightness of the projection field can be made to favor the color in the after-image and to have no effect on the color of the stimulus.

2. No attempt was made to separate the achromatic factors and to study directly the relative importance of their effect upon the frequency with which the after-image may be obtained. However, in her study of the effect of achromatic conditions upon the limits of color sensitivity and upon color quality in stimulus and after-image, some attempt at separation was made, and in these experiments the observer was required to report cases in which color was sensed in the after-image when none was sensed in the stimulus. In order to study the effect of a given factor, the influence of all the factors but the one to be studied should be eliminated from the conditions of the experiment. If, for example, it were wanted to study the effect of the projection field, a gray of the brightness of the colored stimulus should have been chosen for the campimeter screen and preëxposure. In no case was this method of working employed. In her experiments to determine the effect of projection-field<sup>1</sup> the following conditions were used: The campimeter screens were platinum white, and black; the projection fields for each screen were in turn of black, white, and medium gray; and the preëxposure was in each case of the same brightness as the projection field. Since neither the preëxposure nor the screen was chosen of the brightness of the colored stimulus, all the factors were present in each case, instead of one. And in her experiments to determine the effect of surrounding field, screens of white, black, and gray of the brightness of the color were used with preëxposures and projection fields to match the screen in each case. In this case also it will be observed that when the brightness of the screen was varied the influence of neither of the other two factors was ruled out.

3. Her statement of the most favorable brightness conditions for getting the after-image is in complete contradiction to all we have been able to find out directly concerning the phenomenon, or to infer from our investigations of the action of brightness upon color either in the positive sensation or in the after-image. For example, she states that white campimeter screen and a white projection field give the most favorable conditions for obtaining after-images of blue and yellow when no color was sensed in the stimulus. Stating her most favorable conditions for the after-images of red and green, she says: "In several instances in our later work a red after-image has followed an unperceived green when the stimulus was given on a white background and the dark screen [projection field] pushed over the color, and a green after-image was obtained for red and orange when the projection-screen was middle gray or black."<sup>2</sup> The writers were able to obtain after-images of the above colors under the conditions cited by Miss Fernald as most favorable, but instead of finding them to be the most favorable they found them to be almost the most unfavorable. As stated above, their most favorable conditions were black preëxposure, black campimeter screen, black projection field; and their most unfavorable conditions were white preëxposure, white

<sup>1</sup> Fernald, G. M., *PSYCHOL. REV. MONOG.*, 1909, X., pp. 37-45.

<sup>2</sup> *Op. cit.*, p. 82.



campimeter screen, and white projection field. The variation of the factors between these extremes gave the phenomenon with a frequency ranging between maximal and minimal. Working with the conditions she found to be most favorable, Miss Fernald was able to obtain the after-image in only approximately 31 per cent. of the total number of cases.<sup>1</sup> With the conditions we have found to be most favorable the after-image may be obtained in practically every case.

4. Miss Fernald's explanation of her results is also in contradiction to the facts as we find them. (a) She says that the most favorable conditions are gotten when the "projection-screens were determined so as most to emphasize the after-image color and the background least to favor the stimulus color."<sup>2</sup> The campimeter screen, then, that narrows the zone of sensitivity for a given color furnishes the most favorable conditions to be obtained for that color. The white screen is most favorable because "in so far as our results justify any conclusions concerning the color limit they seem to show that all the colors except the reds are perceived at a greater degree of eccentricity with the dark than with the light backgrounds. Red is seen as red to about the *same* degree of eccentricity with the dark and light backgrounds, but it is seen as yellow or orange with the dark background at the same points at which it is seen as colorless with the light background."<sup>3</sup> Her exception in case of red seems due to the fact that the limit for red is for her where first a trace of yellow comes in. If the limit for red had been taken at the point where the last trace of red is seen, as is usually done, red would have proved no exception to the other colors, and we could derive from her results the law that all the colors have a narrower limit with the white screen than with the dark or black screens. We do not find this to be true. For us blue and green have a wider limit with the black screen than with the white, and red and yellow a narrower limit. These results were obtained with the effect of preexposure eliminated and with the illumination of the optics room carefully standardized by a method to be described in a later paper. Neither of these precautions was observed in Fernald's work. Their importance may be shown by the following results. As compared with the gray of the brightness of the color, a preexposure of three seconds to black was found by one of the writers (Rand) to narrow the limits of sensitivity for red, green, and yellow 6°, and for blue 11°; a preexposure to white to narrow the limits for red 5°, for green 2°, for yellow 4°, for blue 7°. At an eccentricity of 35° on the temporal meridian, a preexposure of three seconds to black raised the limen for red 58°, for green 80°, for yellow 32°, and for blue 13°; a preexposure to white raised the limen for red 45°, for green 55°, for yellow 12°, and for blue 7°. When the white campimeter screen was used, the changes in illumination from 11 A.M. until 4 P.M. on a bright day in September were found to vary the limits for the different colors from 4° to 6°. The change in the illumination of our optics room, lighted by skylight and provided with diffusion sashes to lessen the effect of external changes, from a bright morning to a cloudy afternoon was sufficient to vary the limits with a white screen from 6° to 14° for the different colors; and with a black screen from 2° to 7°. The greater variability is found for the white screen because the amount of contrast it induces is found to vary more with the change of illumination. (b) Miss Fernald seems to believe that the brightness process may have either a stimulating or an inhibiting effect on the color process. She says: "There seem to be two possible ways of explaining the action of brightness: Either the brightness of the stimulus has a direct inhibitory or stimulating

<sup>1</sup> Fernald, G., *PSYCHOL. REV.*, 1908, XV., p. 33.

<sup>2</sup> Fernald, G., *PSYCHOL. REV. MONOG. SUP.*, 1909, X., p. 82.

<sup>3</sup> *Op. cit.*, p. 23.

effect on the color processes, or the brightness primarily affects the brightness substance, and the activity in brightness substance has some differential effect on the color activity. . . . The fact that our most striking effects were obtained when the brightness is superimposed on the color, *i. e.*, when the brightness is largely determined by contrast with a brightness background, or when the after-image is projected on a light or dark ground, seems at least to justify the statement that the superimposed brightness acts in such a way as to inhibit, increase, or modify the color activity."<sup>1</sup> Her claim that all the colors have their widest limits with the black screen and their narrowest with the white seems to indicate that she considers that white increases the color activity and that black decreases it. She says: "This brightness factor is effective to a very limited extent in central vision, to a much greater extent in peripheral vision. As we go out into the peripheral retina, the action of the white process is needed more and more, *i. e.*, the colors must be made lighter to be most strongly sensed."<sup>2</sup> Our results show that the brightness process, white, black, or gray, must be considered as always having an inhibitive action on the color processes. When the brightness process is added to the color process either as after-image or contrast, the amount of colored light coming to the eye remains unchanged and yet the saturation of the color is considerably decreased. Instead of being increased by the action of white and decreased by the action of black the saturation is decreased by the action of both, but much the most by the action of white for all colors for all parts of the retina with the exception of for red and yellow within a very narrow zone just within the limits of sensitivity. In this zone red and yellow have a higher limen in black than in white. It is not certain, however, that even in this zone black exerts the greater inhibitive action, for it is difficult to add the same amounts of black and white to red and yellow because these colors darken in the peripheral retina and it is hard to get a measure of how much black is added by this process alone. The relative amounts of inhibitive action by white, black, and gray of the brightness of the color can also be shown by the method of objective mixing. When equal amounts of white, black, and the gray are added in turn to a disc of color on the color mixer, the colored light coming to the eye is reduced an equal amount in each case, yet the apparent saturation of the color is very different on the three discs. It is much the greatest on the disc to which black has been added, next greatest on the disc to which gray has been added, and the least on the disc to which white has been added. Or to get a more exact measurement of the actions in each case, the color limen may be determined. An amount of color which is just noticeable when added to the black disc will give no color at all when added to the white or gray disc. A considerably larger amount must be added to the gray disc to give color, and a still larger amount must be added to the white disc than was added to the gray disc.

Such a confusion as Miss Fernald and many others before her have fallen into with regard to the relation of brightness to intensity or strength of color is apt to come from the failure to separate the action of the brightness or achromatic excitation from the action of physical intensity or energy of the light waves coming to the eye. When a colored light of low energy strikes the eye it is both unsaturated as to color and for the most of the colors of low brightness. As its energy is increased it becomes more strongly sensed as color and in most cases lighter. That the two changes go hand in hand when the physical energy of the colored light is changed, however, does not justify the inference that they will go hand in hand when there is no change in the energy

<sup>1</sup> *Op. cit.*, p. 79.

<sup>2</sup> *Op. cit.*, p. 73.



of the colored light—in other words does not justify the inference that an increase in the white excitation will heighten the color excitation. The central principle of Miss Fernald's method of working is to change the achromatic excitation without affecting the amount of colored light coming to the eye. In all such cases, with the possible exception of red and yellow for the small zone mentioned above, the saturation of the color decreases rapidly as the quality of the achromatic component is made lighter. In addition it is scarcely necessary to point out that just because a color reaches its maximal saturation at a given brightness as the energy of the colored light is increased, it does not follow that the achromatic excitation corresponding to this brightness is the most favorable in its action upon the color process. When the amount of colored light is rendered constant and the achromatic factor is varied, it is found that black is the most favorable quality of the achromatic series and the specific grays are favorable in proportion as they are near to black in quality. The gray of the brightness of yellow, for example, kills out the color in yellow when mixed with it almost as rapidly as does white and much more rapidly than does black and the darker grays. Thompson and Gordon, while in general agreeing with Fernald with regard to the effect of brightness relations, in one place seem both in addition and in contradiction to have fallen into this latter error. They say: "The effect of the background then seems to be this; that in a colored after-image, that color element is emphasized which in brightness approaches the brightness of the background [by background is meant here the field on which the after-image is projected], that is, on the lighter grounds the brighter element comes out and on the darker grounds the darker color element."<sup>1</sup>

By way of clearing the ground for explanation, the writers have made a detailed study of the influence of the various brightness qualities upon color in the central retina, and for a large number of meridians in the peripheral retina. Since a statement of the results obtained will be given in full in a later paper, nothing more than a general statement will be attempted here. In the central retina, white reduces the saturation of colors the most, the grays in the order from light to dark next, and black the least. In the peripheral retina, this law holds for blue and green out to the limit of sensitivity for all of the observers worked with; and also for red and yellow for all of the peripheral retina except a very narrow zone just within the limit of sensitivity. In this zone, black apparently inhibits red and yellow most strongly—at least, red and yellow have a higher limen here when mixed with black than when mixed with white or the grays. An ultimate statement of most favorable conditions for all parts of the retina, then, would have to take into consideration all of the facts. It would involve a more detailed consideration of the topography of the retina than can be gone into in this paper. In formulating our

<sup>1</sup> Thompson and Gordon, *op. cit.*, p. 128.



experimental technique, however, we have avoided, we believe, the difficulty raised by the exception to the law found just within the limit of sensitivity by employing inhibitive conditions sufficiently strong to allow us to work nearer the center of the retina, where the exception has never been found. Thus we have been able to use as our working principle the law that white inhibits the most, grays in the order from light to dark next, and black the least. But to determine in accord with the law even thus simply stated, the brightness quality of preëxposure, campimeter screen, and projection field that will give the conditions most unfavorable for the stimulus color and most favorable for the after-image color, is not easy. For example, in order to inhibit the stimulus color most strongly, the preëxposure must be black, so as to give a white after-image to fuse with the color. This effect could be strongly intensified by having the black preëxposure made through an opening in a white campimeter screen, and the color exposure, which comes immediately after and simultaneously with the after-image of the black preëxposure, made through a black screen. This would secure the greatest possible intensification of the white after-image, and, therefore, the greatest possible amount of inhibition of the stimulus color. In order to favor maximally the saturation of the after-image color, the after-image should be projected on a field of very dark gray or black.<sup>1</sup> As to campimeter screens to be used during the projection of the after-image, we have a choice again of brightness qualities ranging from white to black. White and the grays in proportion to their whiteness would intensify by contrast the blackness of the projection field, while black would exert little if any influence. The black screen, then, is probably the safest to use while the after-image is being observed for two reasons. (1) If one is working with a general illumination at all intensive, white and the grays in proportion to their whiteness induce enormously in the peripheral retina. This amount of induction of black carries the brightness quality beyond that

<sup>1</sup> It may be well to state that in our study of the effect of brightness upon color, the black used was the matt black of the Hering papers. When we state that black favors the saturation of colors, this black is referred to.

specified in our law as most favorable, namely, the blackness of the pigment of the Hering paper. (2) There is always danger that we may not be working far enough within the limit of sensitivity to escape the exception to our law of most favorable action. If, then, our formulation of conditions be correct, we should have a white campimeter screen during preexposure to black, and a black screen during both the exposure of the stimulus color and the projection of the after-image. This would involve two changes of the card behind the stimulus opening in the screen, and one change of the screen. The change of cards causes no disturbance in our phenomenon, but a change of the campimeter screen in the interval between the preexposure and the exposure to color would be fatal to the success of our experiments. This is because the white after-image would not last through the change, for reasons that have already been discussed; and the effect of the preexposure on the stimulus color would therefore be lost. We are thus limited to one screen for a single experiment, and our problem becomes to determine which of the brightness qualities acting continuously through all three stages of the experiment will be the most favorable for our phenomenon. After rough preliminary tests, three screens were selected as representative of the action of all, namely, white, black, and a gray of the brightness of the color to be used. Of these three, the black screen was found to be much the most favorable. A consideration of the action of the three brightness qualities upon preexposure, stimulus color, and after-image shows sufficient reason for this. The effect of each is as follows: (1) The white screen intensifies the blackness of the preexposure, darkens the stimulus card, and, in the peripheral retina, especially if the general illumination is intensive, piles up the blackness on the projection field to a degree that is unfavorable to the saturation of the after-image color. (2) The gray screen of the same brightness as the color adds blackness both to the preexposure and to the projection field, but not so much as is added by the white screen. It has no effect on the stimulus card. (3) The black screen has no effect on the preexposure; it induces white on the stimulus card and thus adds to the

effect of the preëxposure on the stimulus color; and it exerts little or no effect on the projection field. A comparison of these effects shows that the black screen in all probability inhibits the stimulus color more than any of the others, and is less unfavorable in its action upon the after-image color. For this reason, it gives the most favorable brightness conditions for obtaining our phenomenon. Considering all the factors, then, we find that apparently the most favorable combination that can be made for our purpose is black preëxposure, black campimeter screen, and black projection field. The results of our experiments show this to be true. Under these conditions, color was obtained in the after-image in practically every case. The next most favorable condition was given by the white or gray screen with black preëxposure and black projection field. The poorest results were obtained with a white preëxposure and white projection field. With this combination, color could not be gotten in the after-image with any consistency of result, whatever brightness quality was used in the campimeter screen.

Having thus worked our way through an explanation of our phenomenon and a determination of the conditions under which it can best be obtained, we will devote the remainder of the report of the work done by methods (*a*) and (*b*) to a brief review of the results obtained in support of the various points that have been made. Our general thesis was that the phenomenon under consideration is but a special case of the difference in the inhibitive action exerted upon color by the different brightness qualities. Color may be obtained in the after-image when none is sensed in the stimulus, if an unfavorable brightness quality is fused with the stimulus color and a favorable one with the after-image color. In detail, our first point was that for all parts of the retina and for all colors, with the exception of two over a narrow zone just within the limits of sensitivity, white reduces the saturation of color the most, the grays in the order from light to dark next, and black the least. This law was generalized from the results of fusion and limen experiments in a large number of meridians of the retina. Our second point was that the combination of the preëxposure and campimeter screen



most unfavorable to the saturation of the stimulus color was the most favorable condition for obtaining our phenomenon. The effect of preëxposure and campimeter screen upon the saturation of the stimulus color was measured in two ways: (1) by the effect on the limen color; and (2) by the effect on the limits of sensitivity.<sup>1</sup> Data were thus obtained which we could directly correlate with the frequency with which color was obtained in the after-image, and so determine whether our position was correctly taken. The results of this correlation show that, estimated in both these ways, the combination that proved the least favorable to the saturation of the stimulus gave color in the after-image in the largest percentage of cases; and, conversely, that the combination most favorable in its action on the stimulus gave color in the after-image in the smallest percentage of cases. A third point was that a black preëxposure and black screen gave the brightness conditions that were most unfavorable to the saturation of the stimulus color. An estimation of the effect of the different combinations of screen and preëxposure by either of the methods mentioned above brings out this point strongly. The least effect was found when preëxposure and screen were both of the brightness of the stimulus color. A fourth point was that preëxposure provides a stronger means of reducing the saturation of the stimulus color than does the screen. To make the test of this point absolute, the influence of one should be completely eliminated while the influence of the other is being determined. This can not be done. The eye must always have some preëxposure, and there will

<sup>1</sup> See Rand, G., 'The Factors Which Influence the Campimetrical Observation: A Quantitative Examination and Methods of Standardizing.' (In press.)

Of these two tests, the limen test has a much broader application, and measures much more directly what needs to be measured. It has a broader application because it can be made anywhere in the zone of sensitivity. It measures more directly what needs to be measured, because the results obtained show just how much color has to be present under a given condition to be sensed as color, while the results in the method of limits only express in terms of degrees how much the limit of sensitivity has been changed. This is a poor measure of how much the color in the stimulus has been inhibited under a given condition: because, in the first place, it is not a direct measure of this action; and, in the second place, the results obtained cannot be even roughly rendered into terms of direct measurement, owing to the fact that the sensitivity of the retina near the limits does not fall off either gradually or regularly.

always be a surrounding field. The effect of preëxposure, however, can be minimized by choosing it of the gray of the brightness of the color to be used as stimulus, and by making the stimulation to it extremely short. Working in this fashion, we have only a slight local brightness adaptation to modify the color excitation immediately following, the effect of which can be taken as practically negligible. The influence of the screen also can be minimized in a similar way, by having it always of the brightness of the color to be used as stimulus. Isolating the action of preëxposure and screen by this method, we estimated the effect of each in turn upon the saturation of the stimulus color, both by the effect on the limen of color and on the limits of sensitivity. Our results in both cases show that preëxposure can be made much the stronger factor. For example, the limits of sensitivity were never made to vary more than  $4^\circ$  by the most extreme changes that could be made in screens, while it could be varied as much as  $14^\circ$  by changes in the preëxposure. The difference stands out still more strongly in the effect on the limen, as would naturally be expected, since changes in the limen more directly express the differences in the inhibitive action than changes in the limits of sensitivity, as was shown in the footnote, p. 219. A fifth point was that black is the most favorable brightness quality for the projection field. This was shown very clearly by using projection fields of white, gray of the brightness of the stimulus color, and black, with each of the combinations of preëxposure and screen, and comparing the results obtained. In addition, these results, when compared with those obtained by varying the preëxposure and the campimeter screen, show that the brightness of the projection field is a very important factor—much more important than the brightness of the campimeter screen, and just as important, possibly more so than the brightness of the preëxposure.

Results in support of these points in general are given in Table I. In this table is shown the percentage of cases, based on twenty trials, in which color was obtained in the after-image when none was sensed in the stimulus. The object of the experiments was to determine the relative importance of the

three factors, preëxposure, campimeter screen, and projection field. The method of experimenting in the following cases was to keep two of the factors constant and find the effect of varying the third. However, in order to show most effectively the most favorable conditions in decreasing order, the results have been grouped as follows. If the individual, horizontal columns are compared, the effect of campimeter screen is shown. If groups of three are compared, the effect of preëxposure is seen. If compared in groups of nine, the effect of projection is seen.

From the table it will be seen that projection field and preëxposure are the most important factors. Of the preëxposures, black is seen to be the most important. Its effect is greater on green and blue than on red and yellow. This is because the inhibitive action of the white after-image following the preëxposure is greater for blue and green than for red and yellow. The full effect of preëxposure was not obtained in our experiments because with a given black preëxposure we did not work as near the center of the retina as we might have done.

In method (c) also (see p. 208), the vertical campimeter was used to give the stimulus. Black was chosen both for the campimeter screen and for the projection screen in each case. Otherwise the procedure was the same as for after-images in direct vision. The results obtained were also similar, with the exception that not so much decrease of illumination was needed to obscure the stimulus color, and more saturated after-images were obtained. The stimulus time in indirect vision must always be shorter than in direct vision. (From 2 to 3 seconds was used.) This is due to the rapid exhaustion to color in indirect vision. After-images fall off in saturation if exhaustion to the stimulus color is carried beyond the stationary-point.

## 2. Contrast

It was found that contrast could be induced for certain colors when the general illumination was sufficiently reduced to obscure the color in the inducing stimulus. Very strong



TABLE I

SHOWING THE PERCENTAGE OF CASES IN WHICH THE COLORED AFTER-IMAGE WAS OBTAINED WHEN NO COLOR WAS SENSED IN THE STIMULUS, UNDER ALL VARIATIONS OF SCREEN, PREEXPOSURE, AND PROJECTION FIELD

Campimeter Screen	Preexposure	Projection	Red	Yellow	Green	Blue
Black	Black	Black	100	100	100	100
Gray	Black	Black	100	100	80	70
White	Black	Black	50	40	50	40
Black	Gray	Black	40	30	30	20
Gray	Gray	Black	50	30	0	0
White	Gray	Black	50	40	0	0
Black	White	Black	50	30	40	0
Gray	White	Black	45	20	0	0
White	White	Black	45	30	0	0
Black	Black	Gray	40	70	50	40
Gray	Black	Gray	20	40	20	20
White	Black	Gray	20	30	0	0
Black	Gray	Gray	0	0	0	0
Gray	Gray	Gray	0	0	0	0
White	Gray	Gray	0	0	0	0
Black	White	Gray	0	10	0	0
Gray	White	Gray	0	0	0	0
White	White	Gray	0	10	0	0
Black	Black	White	0	0	10	0
Gray	Black	White	0	0	0	0
White	Black	White	0	20	0	0
Black	Gray	White	0	0	0	0
Gray	Gray	White	0	0	0	0
White	Gray	White	0	0	0	0
Black	White	White	0	0	0	0
Gray	White	White	0	0	0	0
White	White	White	0	0	0	0

contrast was aroused under these conditions by standard red, dark orange, and yellow of the Hering series of colored papers.

Contrast discs were cut as follows. The inducing surface was made of two colored discs; one 25 cm., the other 10.5 cm. in diameter. The contrast surface was made by placing between these discs on the color-mixer a black and white disc, 11.5 cm. in diameter. When these were rotated, the black and white mixed to give a gray ring 1 cm. in width, separating the two colored surfaces. The proportions of black and white were taken so that the gray ring matched in brightness the

color used, the brightness of the color having been determined by means of Schenck's flicker photometer.

The observations were made in the dark-room described above. The color of the inducing surface was obscured by a decrease of the illumination. As in the after-image experiments, this was done by two methods, one in which the observation was made after the eye had adapted to the illumination chosen; the other before adaptation had set in. The latter method gave a much stronger effect, for although decrease of illumination within wide limits increases contrast effect in general, this increase is very much greater while the illumination is decreasing. That is, if two determinations of contrast are made for the same inducing and contrast surfaces, one in which the judgment is passed while the illumination is decreasing, the other after the eye has become adapted to the illumination at which the former judgment was made, it will be found that the former determination greatly exceeds the latter. Thus it appears that color induction is greatly enhanced while the retinal change corresponding to dark-adaptation is going on. This phenomenon will be treated more fully in a later paper on contrast. Our present purpose is satisfied with the consideration of the phenomenon at one point in the series of decreasing illumination; namely, the point at which the color in the inducing surface is obscured. This point varies greatly for the different colors used. It is the highest for red, next highest for yellow and dark orange, lowest for green, and next lowest for blue.

The amount of contrast induced in each case was determined by two methods. In each method a measuring-disc was used, compounded from discs of the proper colors and of black and white. In the first method, the comparison-judgment between the contrast ring and the measuring-disc was made at the illumination at which the inducing color was obscured. This method may not be clear to the reader. It may seem, for example, that a degree of illumination that wholly obscures the inducing color would also wholly obscure the color of the measuring-disc. This is not true because of the different effect of the decrease of illumination upon the saturation of the dif-

ferent colors, *i. e.*, blue, green, and blue-green, the colors used for the measuring-discs, retain considerable color at the illumination at which red, yellow, and dark orange, the inducing colors used, lose their saturation. The difficulty with the method is, that the measurements on the comparison-disc at a low illumination are not of standard value because of decrease of saturation, and thus convey little meaning to the mind of the reader. In order to get measurements in standard terms, a second method was resorted to, the results of which are more intelligible although the method of judgment is less accurate. In this method the comparison was made in terms of the saturation of colors on the measuring-disc at full illumination. Since one of the terms of comparison is a memory-image, a time error was involved in this method. This was to some extent compensated for, however, by working both ways, *i. e.*, a part of the judgments were made by first getting a memory-image of the contrast sensation at decreased illumination and comparing that with the measuring-disc at full illumination, and a part were made by the inverse procedure.

The tables recorded in the report are compiled from the results of Misses Chamberlain (*C*) and Rand (*R*), fellows in psychology, of Bryn Mawr College, and Bunker (*B*), graduate student.

### 3. *The Purkinje-Brücke Phenomenon*

The Purkinje-Brücke phenomenon was found by us to demonstrate in a very striking fashion that it is not necessary for the inducing excitation to condition sensation directly in order that color induction may take place.

This phenomenon was first described by Purkinje in 1825.<sup>1</sup> He says: "Man liege ein weisses Quadrätchen von der Breite zweier Linien auf einen schwarzen Grund, starre es 20-30 Secunden an, und blicke sodann ins Schwarze hinein, so wird man ein noch dunkleres Viereck sehen, dessen Randes mit einem graulichen, sich allmählich verlierenden Scheine umgeben sind. Lagt man auf den schwarzen Grund statt des weissen Quadrätchens ein rothes, so zeigt sein grünes Spectrum einen

<sup>1</sup> Purkinje, J., 'Beobachtungen und Versuche zur Physiologie die Sinne,' 1825, II., p. 107.



TABLE II

C. SHOWING THE AMOUNT OF CONTRAST INDUCED WHEN THE COLOR IN THE INDUCING SURFACE IS UNSENSED

Stimulus	Contrast Ring	Direct Judgment	Memory-image Judgment
Red.....	White 41° Black 319°	Green 130° Black 230°	Green 171° Black 119° White 70°
Yellow.....	White 236° Black 124°	Blue 151° Black 130° White 79°	Blue 76° Black 223° White 61°
Dark orange.....	White 82° Black 278°	Green 210° Blue 56° Black 94°	Green 168° Blue 14° Black 172° White 6°

TABLE III

OBSERVER B

Stimulus	Contrast Ring	Direct Judgment	Memory-image Judgment
Red.....	White 41° Black 319°	Green 228° Black 132°	Green 214° Black 94° White 52°
Yellow.....	White 236° Black 124°	Blue 121° Black 142° White 97°	Blue 163° Black 59° White 138°
Dark orange.....	White 82° Black 278°	Green 136° Blue 118° Black 106°	Green 152° Blue 106° Black 102°

TABLE IV

OBSERVER R

Stimulus	Contrast Ring	Direct Judgment	Memory-image Judgment
Red.....	White 41° Black 319°	Green 225° Black 135°	Green 166° Black 126° White 68°
Yellow.....	White 236° Black 124°	Blue 137° Black 189° White 34°	Blue 82° Black 159° White 119°
Dark orange.....	White 82° Black 278°	Green 195° Blue 64° Black 52° White 49°	Green 127° Blue 44° Black 159° White 30°

rothlichen Schein; auf gleiche Weise das blaue Spectrum einen orangen Schein, u.s.w. Man sieht heraus dass die objective Farbe nicht bloss in die Tiefe der Retina, sondern auch in die Breite einwirkt jedoch nicht gleich mässig nach ihrer ganzen Ausbreitung, sondern zunächst an der Gränze der heterogenen Beleuchtungen am intensivesten."

The phenomenon with some modifications was next described by Brücke in 1851,<sup>1</sup> and by Aubert in 1862.<sup>2</sup> The color or added brightness in the after-image was called by Brücke an after-effect of induction;<sup>3</sup> by Aubert an after-image of contrast. Attention was again called to the phenomenon by Hering in

<sup>1</sup> Brücke (*Pogg. Ann.*, 1851, LXXXIV., pp. 418-448) says (p. 47): "Einen dritten Beweis endlich kaum man aus der Beobachtung der negativen Nachbilder entnehmen, welche nach diesem Versuchen zur Erscheinung kommen welche zeigen, dass die inducirten Farben als solche im Stande sind, complementären gefärbte Nachbilder hervorzurufen." His demonstration consists in getting the after-effect of looking towards the light through squares of red, green and violet glass with small discs of black paper placed at their centers. He describes the after-effect as follows: "Bei Anwendung des rothen Glases erscheint als negatives Nachbild eine helle rothe Scheibe auf dunkel grünem Grunde. Hierin liegt nichts Auffallendes und dieser Erfolg würde sich nach Analogie der Versuche von Fechner erklären lassen, auch ohne dass man eine Nachwirkung der inducirten Farbe voraussetze. Wende ich aber das grüne Glas an, so habe ich von der dunklen Scheibe ebenfalls ein helles rothes Nachbild und der Grund ist Schwarz, oder wenigstens so dunkel, dass ich seine Farbe nicht mit Sicherheit habe unterscheiden können. Hier hat also das inducirte Grün Roth hervorgebracht, während das inducirende gleichzeitig kein deutlich gefärbtes Nachbild erzeugte, In derselben Weise zeigte sich mir bei Anwendung des violetten Glases das negative Nachbild als eine gelbgrüne Scheibe auf schwarzem Grunde." In two cases here Brücke apparently has the positive excitation induced across the black disc, and in two cases the negative excitation. But since he does not make induction apply to the negative excitation, as has been done later, he does not explain this case as after-image of contrast, but makes it instead analogous to the phenomenon described by Fechner.

<sup>2</sup> Aubert (*Pogg. Ann.*, 1862, CXVI., pp. 249-279) says on p. 259: "Ausserdem ist es aber auffallend dass der simultane Contrast selbst noch einen successiven Contrast hervorruft, indem die durch den simultanen Contrast complementär gefärbten weissen Quadrate noch einmal complementäre Nachbilder hervorrufen, so dass jene Quadrate im Nachbilde dieselbe Farbe, nur sehr bedeutend abgeschwächt haben wie der Grund." Aubert observed white squares on a colored ground. In the positive sensation the squares took on a tinge of color complementary to the background, and in the after-image of the same color as the background. That is, a white square on a red background appeared greenish in the stimulus and red in the after-image. He says: "Besonders schön und mit einem eigenthümlichen Glanze erschienen die Nachbilder der weissen Quadrate auf dem blauen Streifen."

<sup>3</sup> Brücke applies the term induction to the positive excitation only.

1878.<sup>1</sup> Hering worked with achromatic sensation alone. He also called the phenomenon an after-image of contrast sensation,<sup>2</sup> and upon his observations based his arguments against Helmholtz's theory of contrast. Continuing the discussion, the experiments were extended to color sensations by Ebbinghaus<sup>3</sup> who considered the phenomenon a combination both of after-image of contrast and of contrast induced by an after-image.

The following form of experiment was adapted by us from Ebbinghaus. Squares 20×20 cm. of red, green, blue, and yellow Hering papers were fastened upon neutral gray backgrounds. Passing vertically through the center of these squares, gray strips 2×20 cm. of Hering papers numbers 8, 24, 2, 41, respectively, were pasted. The after-effect of stimulation by this combination is a square of a color complementary to the color of the stimulus square, traversed by a strip of the same color as the stimulus square. The red square, for example, gives a green square traversed by a strongly saturated

<sup>1</sup> Hering, E., 'Zur Lehre vom Lichtsinn,' Wien, 1878, pp. 5-18.

<sup>2</sup> On pp. 5-18 Hering describes experimental conditions similar to those described by Purkinje. Here he calls the phenomenon successive light induction. On pp. 24-29 he describes slightly different experimental conditions. Two dark gray strips of the same brightness, 3 to 4 cm. long and .5 cm. broad, are fastened parallel to each other 2 cm. apart, one upon a white and the other upon a black ground. A fixation-point is taken on the boundary between the white and black backgrounds midway between the strips. By contrast the strip on the white ground looks darker than the strip on the black ground. When an after-image of the strips, the one lightened, the other darkened by contrast, is obtained, their brightness values are reversed, and a still greater brightness difference between them is found. This difference toward black on one hand, and toward white on the other, Hering calls an after-image effect of the brightness contrast induced in the stimulus. The phenomena obtained by these two sets of conditions are in every sense identical. One can scarcely see a reason for the separate treatment they have received by Hering.

<sup>3</sup> Ebbinghaus (*Grundzüge der Psychologie*, Erster Band, p. 239) says: "Man lege zwei mässig grosse Blätter z. B. von sattgrüner Farbe so auf einen grauen Grund, dass nur ein schmaler, etwa 5 mm. breiter horizontaler Streifen zwischen ihnen freibleibt, und lasse diesen von einer unbefangenen Person eine Weile fixieren. Dann lasse man sie das Nachbild auf einem etwas unregelmässig geformten Grunde entwerfen, z. B. auf dem Fensterkreuz, und frage, was sie sehe. Man wird so gut wie ausnahmslos die Antwort erhalten: 'einem grünen Streifen.' Der objektiv völlig neutrale Streifen hat durch die zweimalige Kontrastwirkung (im Vor- und Nachbilde), die sich von einer ausgedehnten Umgebung auf seine schmale Fläche konzentriert, eine so intensive Färbung bekommen, dass er sofort die Aufmerksamkeit auf sich zieht, während die röthliche Nachbildfärbung seiner Nachbarschaft bei den Unregelmässigkeiten der reagierenden Fläche in der Regel gar nicht beachtet wird."



red strip; the green square, a red square traversed by a green strip. Now if the color of the strip is an after-effect of a previously induced contrast, we have a strongly saturated, long-enduring after-image of an unsensed stimulus, for the brightness opposition of the gray strip to the inducing color, and the rather intensive illumination under which we worked, both combined to inhibit all contrast color in the stimulus. At this stage of the work we do not feel prepared to take positive ground on the question of explanation, but have the following evidence to offer that the Brücke interpretation is correct.

*i. Evidence that the Color in the Strip is an After-Image of a Previous Contrast Sensation, rather than Contrast in the After-Image*

(a) In the after-effect, the strip and square apparently develop, fluctuate, and die away independently of each other; the strip frequently develops before the square, especially if the stimulation has been very short; it invariably lasts longer than the square, returning several times after the square has finally disappeared; and in fluctuating, the two figures behave much as two after-images are observed to do, so far removed from each other as to be wholly without the sphere of reciprocal influence. The strip is frequently present when the square has disappeared, and vice versa. It rarely happens that their phases coincide, and when they do, the connection is obviously a chance one.

Records on this and the following points were taken from a number of observers, both experienced and inexperienced. The results given in the following tables are typical. The work was done in a large optics room, lighted on one side by a bank of windows extending nearly to the ceiling. The observer, head in rest, was seated in front of these windows so that the light coming from above and either side fell uniformly upon the projection-field of engine-gray cardboard, 1 meter distant. The time of stimulation, unless otherwise stated, was 30 seconds, and the unit of record was one second. The recording apparatus used throughout consisted of a Ludwig-Baltzar kymograph; a double electro-magnetic recorder, and two con-

tact keys, one for strip and one for square; a Jacquet chronograph set to seconds; and a lamp rheostat to reduce the current from the lighting circuit.

TABLE V

C. SHOWING THE INDEPENDENT PHASES OF VISIBILITY AND INVISIBILITY OF SQUARE AND STRIP

Stimulus	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Red, 20×20 cm. . . .	8	5	6.2	56	7.1	57	113
Gray No. 8, 2×20..	15	6	5	89.5	4	60	149.5
Green, 20×20 . . . . .	2	6	5	15	3.8	7.5	22.5
Gray No. 24, 2×20.	6	7	4.6	32	3.7	22	54
Yellow, 20×20. . . . .	3	17	8	32	11	33	65
Gray No. 41, 2×20.	8	18	6.4	58	2.4	19	77
Blue, 20×20. . . . .	4	12	6.5	32.5	5	20	52.5
Gray No. 2, 2×20..	9	11	6.1	61	3.7	33	94

TABLE VI

OBSERVER B

Stimulus	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Red, 20×20 cm. . . . .	11	12	8.5	102	4	44.5	146.5
Gray No. 8, 2×20..	18	9.5	4.2	79	4	72	151
Green, 20×20. . . . .	4	5	3.1	15.5	4.1	16.5	32
Gray No. 24, 2×20.	6	7	4.4	30.5	3.8	22.5	53
Yellow, 20×20. . . . .	3	3.5	3.1	12.5	7.5	22.5	35
Gray No. 41, 2×20.	7	3	3.1	25	3.3	23	48
Blue, 20×20. . . . .	6	13	4.1	28.5	7.3	44	72.5
Gray No. 2, 2×20..	11	3	3.6	43	3.3	36	79

TABLE VII

OBSERVER R

Stimulus	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Red, 20×20 cm. . . . .	3	5	4.8	19	10.6	32	51
Gray No. 8, 2×20..	7	5	3.5	28	3.3	23	51
Green, 20×20. . . . .	3	2	6.2	25	4	12	37
Gray No. 14, 2×20.	5	7	5.3	32	3.6	18	50
Yellow, 20×20. . . . .	2	12	7.7	23	9.5	19	42
Gray No. 41, 2×20.	7	13	5.5	44	2.3	16	60
Blue, 20×20. . . . .	3	22	9.8	39	4.3	13	52
Gray No. 2, 2×20..	10	7	4.2	46	3.1	31	77

(b) The mutual independence of strip and square can be further indicated by a method of concomitant variations. That is, the strip can be made to fluctuate less and last longer without any corresponding change in the fluctuation and duration of the square. And conversely, the square, or rather what in this variation corresponds to it, can be made to change its duration and rate of fluctuation, without any corresponding change in the fluctuation and duration of the strip. Both of these variations are based upon the effect of the arrangement relative to the direction of greatest involuntary eye-movement, upon the fluctuation and duration of a strip after-image. If the observer has more eye-movement in the horizontal than in the vertical, a strip after-image with its greater dimension in the vertical will fluctuate more and last a shorter time than one of the inverse arrangement. It may be stated as a law<sup>1</sup> that whenever the direction of greatest eye-movement is along the shorter dimension of the after-image, the maximal fluctuation and minimal duration is attained for that form of after-image.

Thus, to vary the duration and rate of fluctuation of the strip without changing them in the square, we need only to arrange the stimulus so that the longer dimension of the strip is first in the vertical and then in the horizontal. This rotation of the stimulus  $90^\circ$  will obviously have no effect upon the duration and rate of fluctuation of the after-image of the square, since both of its dimensions are equal. If we wish to make the converse variation, *i. e.*, change the duration and rate of fluctuation of the outer figure without changing them for the inner, we shall obviously have to make the outer figure a strip and the inner a small square. Then by rotating the stimulus  $90^\circ$  we shall increase or decrease the duration and rate of fluctuation of the outer figure, depending upon whether its shorter dimension is in the vertical or horizontal, while the duration and rate of fluctuation for the inner figure will not be affected.

(c) The Brücke interpretation seems also to receive negative support from the following fact. When one observes the

<sup>1</sup> Ferree, C. E., 'Intermittence of Minimal Visual Sensations,' *Amer. Journ. of Psychol.*, 1908, XIX., pp. 101-103. For explanation of this phenomenon see same reference, pp. 126-127.



TABLE VIII

C. SHOWING BY METHOD OF CONCOMITANT VARIATIONS A DECREASE IN FLUCTUATION AND AN INCREASE IN DURATION OF THE STRIP WITH NO SIGNIFICANT CHANGE IN THE PHASES OF THE SQUARE

Stimulus	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Blue, 20×20 cm.	3	25	15.3	61	5.7	17	78
Gray No. 2, 2×20 (vertical) . . . . .	11	12	6.8	81	3.3	36	117
Blue, 20×20. . . . .	2	48	20.6	62	5.8	11.5	73.5
Gray No. 2, 2×20 (horizontal) . . . . .	9	11	9.4	94	4.3	39	133

TABLE IX

OBSERVER B

Stimulus	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Blue, 20×20 . . . . .	7	6	6.2	50	3.9	27	77
Gray No. 2, 2×20 (vertical) . . . . .	13	4	4.1	57	2.2	29	86
Blue, 20×20 . . . . .	10	13	4.4	48	2.4	24	72
Gray No. 2, 2×20 (horizontal) . . . . .	9	5	7	70	2.9	26	96

TABLE X

OBSERVER R

Stimulus	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Blue, 20×20 . . . . .	3	17	7.5	30	5	15	45
Gray No. 2, 2×20 (vertical) . . . . .	7	7	4.4	35	2.1	15	50
Blue, 20×20 . . . . .	3	14	7.8	31	3.5	14	45
Gray No. 2, 2×20 (horizontal) . . . . .	3	14	10.5	42	3.3	10	52

TABLE XI

C. SHOWING BY METHOD OF CONCOMITANT VARIATIONS A DECREASE IN FLUCTUATION AND AN INCREASE IN THE DURATION OF THE SQUARE WITH NO SIGNIFICANT CHANGE IN THE PHASES OF STRIP

Stimulus	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Blue, 20×.5 (vertical) . . . . .	9	9	4.3	43	1.5	14	57
Gray No. 2, .5×.5 . . . . .	0	9	9	9			9
Blue, 20×.5 (horizontal) . . . . .	5	15	9.7	58	2.4	12	70
Gray No. 2, .5×.5 . . . . .	0	7	7	7			7

TABLE XII

OBSERVER B

Stimulus	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Blue, 20X.5 (vertical) . . . . .	8	6	3.3	30	2.3	18	48
Gray No. 2, .5X.5 .	0	7	7	7			7
Blue, 20X.5 . . . . .	4	10	9	45	2.5	10	55
Gray No. 2, .5X.5 .	0	8	8	8			8

TABLE XIII

OBSERVER R

Stimulus	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Blue, 20X.5 . . . . .	7	5.5	4	32	2	14	46
Gray No. 2, .5X.5 .	0	5	5	5			5
Blue, 20X.5 (horizontal) . . . . .	3	43	17	68	1	3	71
Gray No. 2, .5X.5 .	0	10	10	10			10

stimulus through tissue-paper or under decreased illumination, the relative duration of strip to square in the after-effect is very greatly increased. This should not take place if a color in the strip after-image is induced by the color in the after-image square. The ratio should remain constant or approximately so, *i. e.*, since the intensity of the after-image square has been reduced by weakening the color of the stimulus square by tissue paper, a corresponding weakening would be expected in the color induced in the strip. The observation thus seems to furnish a negative indication that the after-image interpretation is correct. Relative to this observation, however, two points need to be taken into account. (1) An inspection of the tables shows that although there is an increase in the ratio of duration of strip to square, there is an actual decrease in the absolute duration of the strip. This might be supposed to indicate a tendency for the strip to vary with the square and thus to favor the contrast interpretation; but the increase of relative duration is too great for this to be probable. Besides, the decrease in absolute duration can be readily accounted for from the other side by a decreased retinal induction in the

stimulus due to the decreased saturation of the square. (2) The second point is quite aside from differential evidence as between the Brücke and Ebbinghaus interpretations. When the stimulus was observed through tissue-paper or under decreased illumination, considerable contrast color developed in the stimulus, where before there had been none, yet the after-image was less than in the former case. This seems to indicate the following. (a) The contrast sensation is only an equivocal index of the amount of excitation set up on the retina by a neighboring surface. This excitation may under one set of conditions arouse an intensive sensation, and under other conditions be equally strong, at least as far as after-effect goes, and excite no sensation. (b) Brightness opposition inhibits only the contrast sensation. It apparently does not inhibit the corresponding retinal induction due to the neighboring surface, at least not its power to give after-effects. That is, the brightness opposition between the square and strip in the stimulus was greater when the tissue-paper was not used and yet the strongest after-image of the contrast excitation was obtained in this case. These suggestions are thrown out merely tentatively and are meant to apply only within the bounds of the evidence offered.

Since the results are similar for both the tissue-paper device and the decrease of illumination, tables will be given only for the former.

TABLE XIV

C. SHOWING THE PHASES OF INVISIBILITY AND VISIBILITY OF STRIP AND SQUARE AND THEIR RELATIVE DURATIONS, OBSERVED UNDER TISSUE PAPER

Stimulus	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Red, 20×20 . . . . .	0	3	3	3			3
Gray No. 8, 2×20 . .	3	4	2.5	10	3	9	19
Green, 20×20 . . . . .	0	7	7	7			7
Gray No. 24, 2×20 . .	2	5	3.3	10	6.5	13	23
Yellow, 20×20 . . . . .	1	8	4.8	9.5	3	3	12.5
Gray No. 41, 2×20 . .	4	8	3.8	19	3.6	14.5	33.5
Blue, 20×20 . . . . .	1	5	6	12	6	6	18
Gray No. 2, 2×20 . .	4	3	2.3	14	2.5	10	24



TABLE XV

OBSERVER B

Stimulus	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Red, 20×20.....	0	2	2	2			2
Gray No. 8, 2×20..	3	1	1.9	7.5	4.8	14.5	22
Green, 20×20.....	0	7	7	7			7
Gray No. 14, 2×20.	2	4	4	12	5.8	11.5	23.5
Yellow, 20×20....	0	4.5	4.5	4.5			4.5
Gray No. 41, 2×20.	3	4	3.1	12.5	4	12	24.5
Blue, 20×20.....	1	3	2.5	5	7	7	12
Gray No. 2, 2×20..	7	4	3.5	28	3.1	22	50

TABLE XVI

OBSERVER R

Stimulus	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Red, 20×20.....	0	3	3	3			3
Gray No. 8, 2×20..	3	3	2	8	3.3	10	18
Green, 20×20.....	0	8	8	8			8
Gray No. 14, 2×20.	3	4	3.5	14	3.3	10	24
Yellow, 20×20....	1	6	4.5	9	2	2	11
Gray No. 41, 2×20.	3	5	4.5	18	4	12	30
Blue, 20×20.....	1	5	4	8	5	5	13
Gray No. 32, 2×20.	4	5	4.6	23	3	12	35

ii. But if a Contrast Effect, Evidence that it May Take Place when the Inducing Color is Unsensed

As already stated, the strip frequently develops before the square; it invariably lasts longer than the square; returning several times after the square has finally disappeared; and in fluctuating its phases rarely coincide with those of the square, *i. e.*, it is frequently visible when the square is invisible, and conversely.

Thus it is immaterial for our thesis which interpretation be given to the phenomenon. For (*a*) if the after-color in the strip be a contrast sensation, our results show that it may be set up when the inducing excitation is not directly conditioning sensation; and (*b*) if it be an after-image sensation, they show that it may be aroused by a previous excitation which did not itself directly give rise to sensation.



FIG. 1. (Observer R.) Records showing the fluctuations and duration of strip and square.

## III. EXPLANATION

A later paper will contain a history of the observations on the effect of brightness changes, general and local, upon color phenomena, beginning with Purkinje.<sup>1</sup> Purkinje's observations cover the following points. (1) The relative difference in the brightness values of the spectral colors at full and decreased illumination. (2) The difference in the effect of change in brightness upon the saturation of colors. (3) The changes in color-tone produced by changes in brightness.

*After-Image*

The explanation of the possibility of getting color as an after-image from a stimulus in which no color can be sensed rests in general with the second point of Purkinje's observations; namely, the difference in the effect of change in the brightness of different colors upon their saturation. In every case in which a colored after-image was obtained from a colorless stimulus, it was gotten at a degree of brightness which worked against color saturation in the stimulus and relatively favored it in the after-image. Take, for example, the case of central vision. By the first method the influence of the brightness factor was introduced by means of a decrease in the general illumination. Yellow and red and orange lose their saturation at an illumination that permits of a supraliminal saturation of blue and green. The case, then, is simple. There was retinal excitation in the cases of red, yellow, and orange, but it was obscured for sensation by the brightness factor. The after-effect of this excitation, however, was not obscured for sensation. It was relatively favored, and, therefore, gave the after-images which were observed. There is nothing new or strange in principle about this phenomenon. The foregoing results show that it depends entirely upon the effect of the brightness changes in a color upon its saturation. These effects were observed and reported as far back as the time of Purkinje, and have been discussed sporadically in the literature from that time to this. By the second method the unfavorable

<sup>1</sup> Purkinje, 'Beobachtungen und Versuche zur Physiologie der Sinne,' 1823, I. p. 109.



brightness quality was added to the stimulus color by objective mixing, contrast, or after-image; and the favorable brightness quality was added to the after-image by means of the projection field. The after-image method was found to be the most effective for adding the unfavorable brightness quality to the colored stimulus because by means of it (*a*) the amount of colored light coming to the eye is not reduced as it is by the method of objective mixing; and (*b*) the unfavorable brightness quality added to the stimulus color is not added to the after-image color also, as is done under the conditions of the experiment by the method of contrast.

For the after-image in peripheral vision, we have a slightly different case. The peripheral retina differs from the central with regard to the effect of change in brightness upon both the saturation and the quality of colors. With regard to saturation, we have in general merely an exaggeration of the condition found in the central retina, *i. e.*, brightness changes produce greater difference in effect in the case of the different colors. With regard to color tone, the change is not in the same direction in every case as it is in central vision, *i. e.*, in central vision at full illumination. In this paper, however, we are concerned with the effects upon saturation alone. Moreover, under a given set of conditions more of the brightness quality can be added as after-image and contrast in this region of the peripheral retina than in the central retina because of the increased sensitivity of the former to achromatic after-image and contrast. We have then in these regards especially favorable conditions in the peripheral retina for obscuring stimulus color by brightness changes, and for relatively favoring the development of the after-image. We have as an additional factor the enhanced sensitivity of the peripheral retina to adaptation and after-image effects. It adapts very rapidly to color stimuli and responds quickly with a vivid after-image of short almost momentary duration, often described as a vivid flash of color. All of these factors make it comparatively easy to get after-images of unsensed stimuli in peripheral vision, the only fact relative to our problem that needs to be explained.

### *Contrast*

It was found to be especially easy to arouse green, blue-green, and blue as contrast sensations when their inducing stimuli do not directly excite a sensation of color. Two factors are involved in this result. (1) A decrease of the illumination obscures red, orange, and yellow before it obscures their contrast colors, green, blue-green, and blue. Hence, induction and the effect of decrease of illumination upon it aside, there is reason in the nature of the color processes themselves why red, for example, should not be sensed when its contrast color, green, is sensed. But (2), in addition to this, decrease of illumination enormously enhances the induction of the contrast color, this effect being greater for green, blue, and blue-green than for their complementary colors red, yellow, and orange.

### *The Purkinje-Brücke Phenomenon*

Until a final decision has been made between the after-image and the contrast interpretations, the Purkinje-Brücke phenomenon presents a two-fold problem. From the side of the after-image interpretation it must be explained how an after-effect so intensive and of such long duration can be obtained from a stimulus of so little apparent intensity; *e. g.*, in our form of the experiment, no color at all could be sensed in the strip. From the side of the contrast interpretation, it must be shown how contrast color can be gotten in the after-effect of the strip when the square is not in sensation; *i. e.*, before the square appears, in the invisibility phase of its intermittence, and after its final disappearance.

At this stage of the work, an explanation will not be attempted. From the Brücke side, however, it may be pointed out that evidence has already been given that color in the contrast sensation is only an equivocal index of the actual amount of the corresponding retinal color excitation. Also, that, while this induced excitation may arouse a strong sensation, a weak sensation, or even none at all, depending upon concomitant brightness conditions, contour, etc., nevertheless, in all of these cases, it gives rise to after-sensations of color, and to the most intensive in the cases we used, when no color was sensed in the

stimulus. In fact, it was just to take advantage of this point that our method was devised. We introduced brightness opposition between strip and inducing color to prevent the induced excitation from arousing a sensation of color, knowing that its power to condition color in the after-image was not diminished thereby, providing the brightness conditions were favorable for the development of the color. The brightness conditions were made tolerably favorable by choosing a shade of gray for the stimulus strip whose after-image was approximately the brightness of the after-image color. From the side of the Ebbinghaus interpretation, the writers have not at present even a plausible suggestion to offer in explanation. However, lack of explanation of this phenomenon does not limit its confirmation of our thesis that stimuli in which no color is sensed may arouse after-image and contrast sensations in which color is sensed.

We wish to state in conclusion that our chief interest in the problem has been its share in the broader problem presented by the Purkinje observations. We do not think that sufficient attention has been given to the second and third of these observations and the light they may throw on color theory. Up to this time, theory has practically ignored the effect of brightness changes upon the saturation and quality of color sensation. These effects seem to argue a functional connection between chromatic and achromatic processes which should not be disregarded.



## A NEW LABORATORY PENDULUM<sup>1</sup>

BY KNIGHT DUNLAP

Having need of a large pendulum for various sorts of work, and finding none of the instruments in the market exactly suited to our purposes, we have had constructed the instrument represented schematically in Figs. 1 and 2. It was built in the workshop of the Johns Hopkins physical laboratory by Mr. Childs, has been in daily use for nearly a year, and has proved very satisfactory. Figs. 1 and 2 are approximately  $1/12$  diameter.

The essential framework is composed of hot-rolled bar steel, with a cast iron headpiece (S). The head-piece is a bottomless box with sides (Fig. 1)  $14 \times 5$  inches, and ends (Fig. 2)  $12 \times 5$  inches, with walls  $11/16$  of an inch thick. At the top, along each side, is a flange extending inwardly  $5/8$  of an inch; giving therefore two top surfaces each  $1\frac{5}{8}$  inches wide, which are accurately planed for the seating of the knife rests, and for other attachments which are to be added later.

The base pieces (Z, Z), are  $1 \times 2\frac{1}{2}$  inches in cross section, and 24 inches long. At each end they are provided with levelling screws (not shown). These levelling screws are lag screws,  $3/4 \times 3\frac{3}{4}$  inches. The four oblique pieces (V, V) are  $1 \times 3/4$  inches in cross section, 52 inches long. These are fastened to the head piece (S) with two  $1/2$  inch lag screws in each, and are bolted to the horizontal piece (X, X) with  $1/2$  inch bolts. The horizontal pieces, (X, X), in turn are fastened to the base pieces (Z, Z) with two  $3/8$  inch lag screws in each end.

The frame as described is very rigid, and not excessively heavy. Two men can easily move it.

The pendulum rod (R) is of  $5/8$  inch round steel, 48 inches long. There are two bobs (P), of cast iron, one weighing about 25 lbs. and the other about 10 lbs. The lighter is used

<sup>1</sup> From the psychological laboratory of the Johns Hopkins University.

in reaction-time work, in which only a single swing of the pendulum is necessary. The bobs are circular in horizontal cross section, and of such shape in the vertical section (very

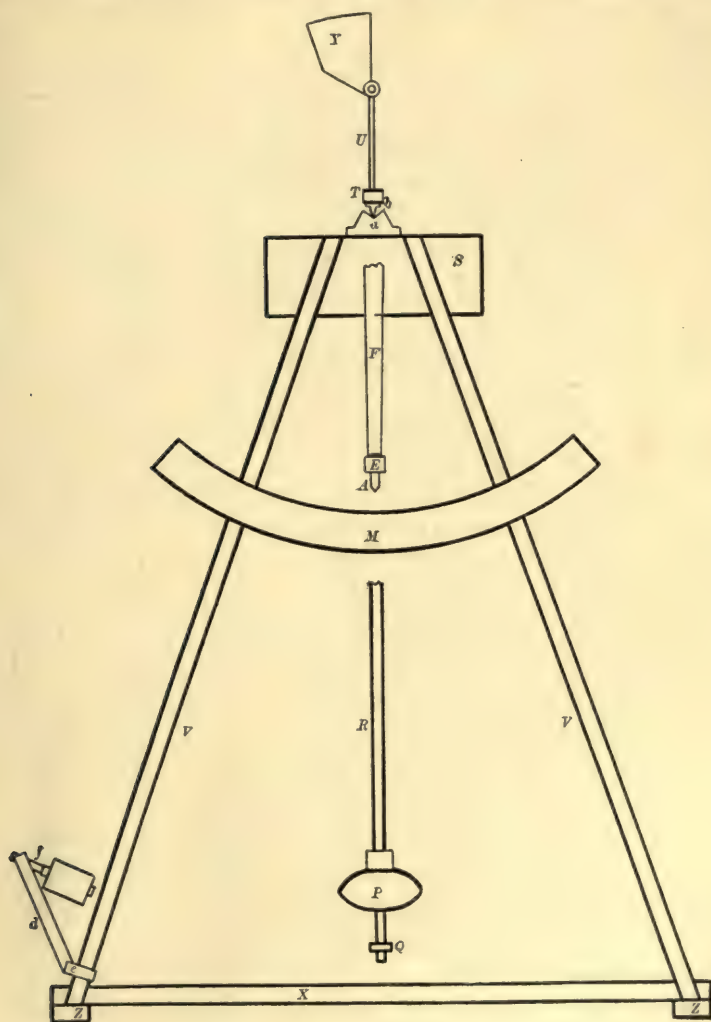


Fig. 1

rudely represented in the figures) that low resistance is offered by the air. The bob is fastened to the rod by means of a set screw (with square head for wrench) and is screwed into the rocking bar (*T*) and locked by a nut. The bar (*T*) is  $1 \times \frac{3}{4}$

inch in cross section, 14 inches long; on it are mounted the knives (*b, b*), which are of tool steel, planed accurately and case hardened, as are also the rests (*a, a*). Lag screws are used to fasten the knives to the bar (*T*), and the rests to the headpiece (*S*).

Near the end of the rod (*R*) is mounted (adjustably) an armature (*Q*) of soft iron  $\frac{1}{2} \times 1\frac{1}{2} \times 2\frac{1}{8}$  inches, by which the electro-magnet (*c*) holds the pendulum deflected. This magnet, by means of the clamp (*e*), the adjustable swing (*d*), and the rod (*f*) which slides through a clamp, is adjustable to hold the pendulum at any point up to thirty degrees from the center. As the knives are planed to thirty degrees, and the rests to ninety degrees, this is the maximal swing.

The arc (*M*) is of cast iron, dressed on a lathe to approximately  $2\frac{1}{2} \times \frac{3}{8}$  inch in radial cross section, bolted to two clamps adjustable on the oblique pieces (*V, V*, Fig. 1), so that it may be accurately centered on the knife edges. In Fig. 2 is shown the projection of the vertical radial cross section of the arc. On this arc (*M*) are supported the contact devices, which are not represented in Figs. 1 and 2, but are shown at approximately  $\frac{1}{2}$  diameter in Figs. 3 and 4.

Each contact device involves essentially a small permanent bar magnet (*G*), held in a brass pinion working in cone bearings. The lower end of the magnet (*G*) has platinized contact surfaces, working against the platinized tips of a contact screw on each side. One of these screws (*r*) is indicated in Figs. 3. These screws may both be of untempered steel, in which case the end of the magnet (*G*) will adhere to either when moved to it; or one may be of steel and the other of brass, in which case the magnet when released will always return to the steel screw. The strips (*H, J*) which carry the contact screws are insulated from the Y-piece (*O*) which supports the bearing of the magnet (*G*), and are provided with binding screws (*s, t*) through which connection may be made with either. A binding screw (*K*) in (*O*) provides for the other terminal. The current passes through the bearing of the magnet (*G*), but if this is not oiled and is kept clean, there is no trouble. It would, however, be better for general purpose



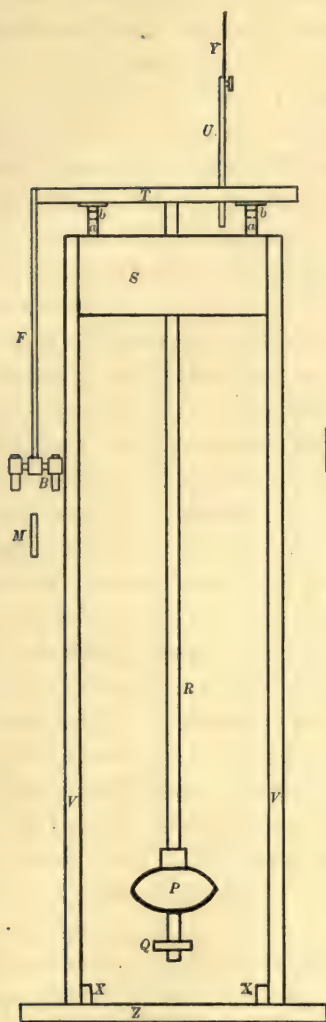


Fig. 2

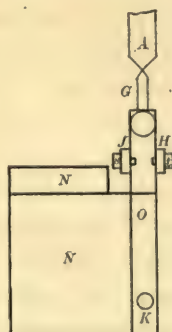


Fig. 4

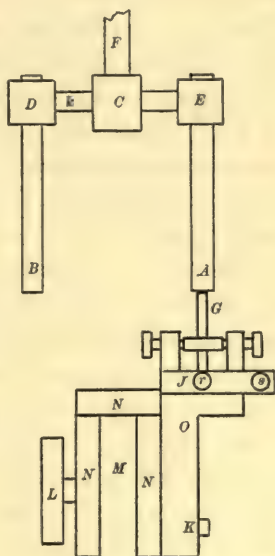


Fig. 3

to connect the pinion of (G) to (O) by means of a flexible conductor.

The small magnet (G) is moved by a master bar-magnet (A) carried by the arm (F) which is attached to one end of the rocker-bar (T). This magnet (A) is adjustable in the sup-

porting clamp (*E*), which is adjustable on the rod (*h*). Thus, the magnet (*A*), carried by the arm (*F*), may be so adjusted that it comes close to the magnet (*G*) without touching it. The upper end of (*G*) and the lower end of (*A*) are beveled at the apposed ends to a width of  $1/32$  inch.

The arm (*F*) carries another magnet (*B*) similar to (*A*). By reversing the contact device so that the clamp screw (*L*), is on the right, instead of on the left, the magnet lever (*G*) is brought under the master magnet (*B*). The contact devices are accordingly all made alike, and owing to the relations of the parts (*N*) and (*O*), two of them may be so adjusted that the contacts are simultaneously operated. The contact devices, except for the magnet (*G*) and the contact screws mentioned, are entirely of brass; as is also the arm (*F*) and the clamps which hold the large magnets (*A*) and (*B*). The parts (*C*, *k*, *h*) are planed and turned from one piece.

For certain exposure work, a screen is attached above the pendulum. The rod (*N*) is arranged to slide through a hole in the rocker bar (*T*) and to be fastened at the desired height by a set screw. The upper end of the rod carries a clamp in which can be secured a screen (*Y*) of aluminum, rubber, cardboard, or other suitable material, having properly cut apertures or edges. With the arrangement shown in Figs. 1 and 2, the screen exposes a light when the pendulum reaches a certain prearranged point in its swing from the magnet (*c*), and recovers it at the same point in its return. Exposures can be arranged below the bearings, by using a different type of pendulum rod.

It is probable that we will modify the instrument by placing the arc (*M*) inside the frame, instead of outside, as at present; and attaching the master magnets by a clamp directly to the pendulum rod at the proper height, doing away with the arm (*F*). The arm was attached because we intended to use a different type of contact, but found that it was too noisy. We planned to use electro-magnets to actuate the described contact devices, but have found the permanent magnets satisfactory for present purposes. These are readily kept at full strength by applying a powerful electro-magnet from time to time; not necessarily every day, although this is preferable as a routine matter.

A further improvement in the contact devices consists in the use of brass contact screws only, and the placing of the steel screws (which may be hardened in this case) just above these. The magnet-lever in this case need not touch the steel screw, which can be adjusted to give just the amount of pull required.<sup>1</sup> The simpler device has however worked well so far. These contacts are far superior to any others with which I am familiar; the reduction of noise effected by avoiding the striking of the pendulum attachment on the contact device is in itself of the highest importance.

Some tests have been made on the capacity of the instrument, but as it has been in constant use in a reaction-time experiment these tests have not been exhaustive. With the 25 lb. weight, the pendulum will swing for several hours after being released (the total time depending on the initial amplitude). If started with nearly its full amplitude, and a period of about 2 seconds (*i. e.*, 2 seconds to a complete swing: commonly called a period of 1 second), the period shortens by nearly fifteen sigma at the end of three hours. The greater part of this change occurs in the first hour, so that by starting with a smaller deflection, it is possible to obtain less than two sigma change ( $1/10$  of 1 per cent.) in a half hour. In the reaction time work, since the pendulum is recaught by the magnet at the end of each swing, all swings are under the same conditions, and exact accuracy is obtained.

Full tests on the pendulum will be made as soon as it is released from the present experiment, and the detailed results will be reported in connection with another piece of work.

<sup>1</sup> This type of contact we have found necessary on the Schumann time-machine, on which it works admirably; even at rapid rates of rotation, when none of the ordinary forms of contact devices work at all.



## DISCUSSION

### CAN BIOLOGY AND PHYSIOLOGY DISPENSE WITH CONSCIOUSNESS ?

"While the comparative psychologists debate concerning the amount of sensation, memory, reflection, that one should attribute . . . to animals, there (arises) in the growing science of comparative physiology an enemy to the death, of all comparative psychology."<sup>1</sup> So writes J. von Uexkuell, careful worker for the last twenty years on the physiology of animal behavior. The strife between the two sciences can only end, he adds, 'in the complete annihilation of one of the two combatants' for, "before objective investigation, the sensations, the memory and thoughts of animals disappeared like fluttering forms of vapor. The iron chain of objective changes, which began with the stimulation of the sense organ and finished with the movement of the muscle, was welded together in the middle. Nowhere remained a smallest spot for the psyche of the animal. Basing itself on these incontestable facts comparative physiology pronounced the psychological conclusions mere superstitions, and denied comparative psychology the right to call itself a science."<sup>2</sup>

A. Bethe, the eminent neurologist and student of animal behavior, with Beer, Ziegler, Nuel and others, is of the same opinion. Conscious states *may* exist but it is not probable; for scientific procedure they are then to be denied. "Chemico-physical processes and their consequences, that is the objective aspect of psychic phenomena, and these alone, should be the object of scientific investigation."<sup>3</sup>

J. Loeb, in his most recent article, writes: "The contents of life . . . are wishes and hopes, efforts and struggles . . . disappointments and suffering. And this inner life should be amenable to a physico-chemical analysis? In spite of the gap which separates us today from such an aim, I believe that it is attainable."<sup>4</sup>

<sup>1</sup> 'Psychologie u. Biologie in ihrer Stellung zur Tierseele. Asher u. Spiro: Ergebnisse der Physiologie,' I. Jahrg., II. Abth., V., p. 213; tr. by H. S. Jennings.

<sup>2</sup> *Ibid.*

<sup>3</sup> 'Die anatomische Elemente des Nervensystem u. ihre physiologische Bedeutung,' *Biol. Cent.*, Bd. 18, p. 864.

<sup>4</sup> *Pop. Sci. Mo.*, Jan., 1912, p. 19.

Other naturalists and physiologists are less extreme. So Claparède, Forel and Wasmann are willing to grant the possibility of a psychic life to lower animals. The physiologist Nagel was of the same mind. Jennings, who has of course psychological interests also, while not apodictic, is wholly favorable to the hypothesis. While he asserts: "There are no processes in the behavior of organisms that are not as readily conceivable without supposing them to be accompanied by consciousness as with it," he says further: "The writer is thoroughly convinced after long study of the behavior of this organism, that if amœba were a large animal so as to come within the everyday experience of human beings, its behavior would at once call forth the attribution to it of states of pleasure, and pain; of hunger, desire and the like on precisely the same basis as we attribute these things to the dog . . . objective investigation is as favorable to the general distribution of consciousness throughout animals as it could well be."<sup>1</sup>

The conclusion one reaches on reading the opinions of these workers in the field of animal behavior, whether their viewpoint be largely descriptive and mechanistic, or functional and vitalistic, is that consciousness is regarded by them as a *Begleiterscheinung*, an epiphenomenon, something pretty vague; a concept that biology can well do without.

For this conclusion psychologists are, I believe, chiefly to blame.

James's well-known definition of psychology is 'the description and explanation of the *states of consciousness as such*.'

Judd defines consciousness as that which 'each one of us has when he sees and hears, when he feels pleasure or sorrow, when he imagines or reasons, or decides to pursue a line of action' and adds: "One hardly knows how to find phrases in which to answer those who hold consciousness to be less real and potent than physical forces. Certainly nature has protected and conserved consciousness throughout the whole development of the animal kingdom. Certainly the world is different because consciousness has been evolved. Certainly consciousness is no less real than are its conditions, and finally consciousness is certainly much more directly approachable to the student of science than is matter."<sup>2</sup>

Titchener uses clearer language: "Mind is the sum-total of mental processes . . . and processes implies that our subject matter is a stream, a perpetual flux, and not a collection of unchanging objects."

<sup>1</sup> 'Behavior of Lower Organisms,' pp. 336 and 337.

<sup>2</sup> 'Psychology,' pp. 13, 62-63.

The definition that consciousness is the mind's awareness of its own processes 'we must reject,' says Titchener. 'This awareness is a matter of observation of the same general kind as observation of the external world,' and the former notion is misleading 'because it suggests that mind is a personal being instead of a stream of processes.'<sup>1</sup>

Between defining consciousness as a *state*, and defining consciousness as a *process*, psychology has neither itself been clear as to the nature of consciousness, nor given a workable definition to students of animal behavior. In consequence such men as Bethe, Beer, von Uexkuell, men who are distinctly physiologists and have no immediate psychological interests, see in the concepts of psychology only an illusive subjective nomenclature that is both inadequate to throw further light on their strictly biological problems, and, what is more, is directly confusing. To ask whether animals have conscious *states*, whether they reflect upon their own processes as they occur, is an irrelevant, because an inconceivable hypothesis. Physico-chemical explanations, while not yet illuminative of life phenomena as such, have a very direct answer to the question of fixed behavior as the expression of life phenomena. So, for instance, tropisms are reducible to the same physical reagents that cause oscillations, tensions, and mutations in chemical processes, digestion and so forth. A fly crawls toward the light, or warmth, or the odor of decay, in a perfectly predictable way. To postulate an accompanying conscious state, does not add, but rather subtracts from our conception of its behavior.

Now much if not most of this confusion will be obviated if psychology will remain consistent to the position stated by Titchener, that consciousness is a *process*. Consciousness is a process in precisely the same sense that osmosis or alimentation is a process.

The writer believes that this position will be clarified when we overcome what amounts to a real handicap in our present psychological terminology. Let us use the term *consciousizing* for this process, and relegate the substantive term consciousness or conscious state to the realm of pure concept. The gain in conciseness will at once appear, if we now ask, not are animals conscious, but does their behavior indicate *consciousizing*. We know that metabolic processes are constantly occurring in the organism; are some of these processes *consciousizing* processes? And by a *consciousizing* process we shall mean not any sort of immediate reflection upon the inner life of process and change, but that process and change itself in so far as it involves a reference to the past experience of the animal, and a modification

<sup>1</sup> 'A Text-book of Psychology,' pp. 16 and 18.



of otherwise rigid behavior in terms of that experience. In this latter case, processes that ordinarily have a simple chemical or physical explanation are obviously inadequate: reaction is to a present stimulus *plus*, and this refined physiological process, this physiological process *plus*, may properly be described, I believe, as a *consciousizing process*. Reflex and mechanical it still is—the dichotomy has never properly been *conscious vs. mechanical*, but always *conscious-mechanical vs. unconscious-mechanical*—but the differentium is this sensitivity to past processes, with the result showing in a modified, more closely adaptive behavior. Once assimilated, consciousizing processes will lapse in favor of purely physiological non-consciousizing processes until such time as new needs arise demanding again the application of old experience to their solution.

Behavior will then have a three-fold description, whether phylogenetically or ontogenetically considered. We shall speak of *pre-consciousizing*, *consciousizing*, and *consciousized* behavior. Pre-consciousizing behavior will be the purely reflex mechanism in action. Infusorian life is largely, if not wholly such. Consciousizing behavior will be characteristic of all races and individuals where development occurs. Such stages are conspicuous in that branch of the zoölogical tree from planarians to man, increasingly exemplified with increase of nervous tissue and integrative mechanisms. Lastly the consciousized organism will be marked by relatively rigid, habitual, instinctive, non-progressive behavior. Such consciousized behavior the hymenoptera of all animals best illustrate: a remarkably efficient but rigid community life, with however, if we be allowed speculation, significant traces of past consciousizing processes, that, having accomplished success in adapting the organism to its environment, have now lapsed from disuse, leaving a splendid monument to their efficiency in an intricate social life—chains of physiological reflexes so rigidly conforming to type, as to exhibit little or no trace of the original consciousizing process.

These distinctions appear even more significant when applied to human consciousness. While it is probably true that pre-consciousizing behavior cannot absolutely be asserted in so actively developing an organism as the human species, nor on the other hand can we point to absolutely consciousized behavior, yet the differences between individuals in this respect, and indeed the differences in a single individual, viewed at different times in his life history, are sufficiently marked. The man in relatively quiet, stable environment lives a non-consciousizing existence for the most part. His vocabulary is

limited to less than a thousand words, his needs are few and to them he becomes early habituated. New ideas, new problems, new motor expressions are rare with him, and when presented, are either ignored, or crudely met, because of lack of that apperceptive mass of past experience sufficient to allow consciousness, *i. e.*, assimilation of the new by the old. The recluse or rustic in the midst of busy city life is the stock illustration. The urbanite is, other things equal, a consciousness individual par excellence. With the latter, streets must be crossed, traffic dodged, business appointments kept, while sounds, sights and disturbances of one sort or another bombard his nervous system from morning till night.

In the individual, childhood years are the pre-consciousizing years. With adolescence and its problems the consciousness process is at its maximum, making way finally in adult life for a body of habits and fixed reactions, that demand less and less attention, and so far as they thus become rigid, are properly described as consciousness behavior.

To substitute the neologism 'consciousizing' for the conventional 'conscious state' will not solve the problem of what the process itself is. We have not, and cannot explain the consciousness process. Neither can we explain a physico-chemical process. Why hydrogen combines as it does with oxygen to form water; why red is red and not green; why protoplasm is irritable anyhow, these are ultimate questions whose answer is locked up with the secret of life itself. Our knowledge here is profound, and a new definition is no adequate explanation. When we can understand how life goes on, what the processes of anabolism and katabolism are, we may expect to find that we have at the same time solved the question as to what the consciousness process is. At present we can detect it, not explain it. As an organism learns, improves, profits by past experience, we are justified in asserting that it is undergoing a consciousness process; that its past experience is being in some effective way, brought to bear upon the solution of present stimuli.

Consciousizing is then a remedial process, aroused only in the presence of some need, some ill-adjustment of organism to environment, acting against an obstacle or inhibition, and its success is marked by the disappearance of both problem and process. Its biological explanation is the inadequacy of purely physiological processes with their fixed mechanisms, to solve new situations. Their paths are canalized and rigid. Its neural basis is a plastic protoplasm, that is more than a vehicle, rather a store of potential energy, guiding

and influencing by facilitations and inhibitions the stimuli that set it off and that are the occasion of its functioning. It is better described as focalized memory than as focalized attention.

Present-day psychology is inconsistent, and so far biologists have just grounds for reproach, when it describes its chief method as introspection, in the ordinary connotation of that term. If we agree that to define consciousness as the mind's awareness of its own processes, is untenable; introspection as a psychological method becomes a misnomer. Self-observation can consist only in viewing my own immediate past instead of yours, or instead of some external fact or thing. My present consciousnessing process can become aware of itself only when codified, to so speak, within the mass of past experience. It may then present itself, and does do so, as a problematic object for a still later consciousnessing process, the latter as yet unaware of itself. The current uncorrected notion that we can grasp the momentary experience as it passes is the arch illusion which contemporary psychology allows itself.

The concept of consciousness, or the concept of the self remain unimpaired. They are as valuable and as unreal as the electron of physics. Such an entity as the self or as the electron may exist. To postulate them as concepts is both possible and wise; to treat them as real things of mass and objectivity is confusing. Of such concepts biology and physiology have no need. Behavior can be everywhere explained without invoking conscious selves. But neither biology nor physiology can dispense with consciousnessing processes which are as real and as universal as are growth, or development, or evolutionary processes.

By strict adherence to this dichotomy of pure concept on the one hand and process on the other, a position not only creditable but alone possible for a self-consistent psychology, physiology will no longer feel itself called upon to build up an artificial system of new terms to explain behavior. Such a make-shift as that attempted by Bethe, Beer, and von Uexkuell, is as unfair to physiology as it is to psychology. Loeb will no longer call consciousness a metaphysical term, and we shall accept his 'associative memory'<sup>1</sup> as a fair synonym for 'consciousizing process.' With von Uexkuell, who in his latest paper attempts to make provision for a modification of the machinery of behavior through the experiences of the organism, and who builds up his explanatory system about the

<sup>1</sup> 'Comparative Physiology of the Brain and Comparative Psychology,' pp. 12-14, 214 et seq.; 236 et seq.



concept of tonus, we shall find ourselves in no necessary disagreement. Of tonus he says: "Es stellt sich der Tonus als eine Energieform dar, die . . . immer vom Orte hoeheren Tonus zum Orte niederen Tonus abfließt." Further, it is ". . . denjenigen Theil der Lebensintensitaet der einzelnen Zelle der . . . dem Gesamtorganismus zur Verfuegung steht."<sup>1</sup>

One may still preserve his psychological integrity and with Bethe agree: "Jede Association ist Folge einer eben vor-aufgegangenen Erinnerung; jede Erinnerung Folge einer voraufgegangenen Wahrnehmung; jede Wahrnehmung verursacht durch einen aeusseren Reiz, und so ist auch die letzte Schlussfolgerung einen langen Gedankenreihe mechanisch verursacht durch einen aeusseren Reiz. Unvermittelt auftauchende Erinnerungen und Gedanken giebt es nicht. Ohne aeusseren Reiz ist auch. . . Erinnerung und Gedankenarbeit unmöglich. . . ."<sup>2</sup>

Our consciousnessing processes could be so characterized. Energy is stored in some modified fashion by past experience; it is put in action by the stimulus now affecting the organism; its result is to modify the machinery of behavior in terms of that past experience. This is what and this is all psychology can mean by conscious *processes*.

ELIOTT P. FROST

YALE UNIVERSITY

<sup>1</sup> 'Die anatomische Elemente des Nervensystems u. ihre physiologische Bedeutung,' *Biol. Cent.*, Bd. 18, p. 868.

<sup>2</sup> 'Die Wirkung von Licht u. Shatten auf die Seeigel,' *Zeit. f. Biol.*, Vol. 40, p. 474.

# THE PSYCHOLOGICAL REVIEW

---

## THE QUESTION OF ASSOCIATION TYPES<sup>1</sup>

BY FREDERIC LYMAN WELLS

In dealing with free association material it is often necessary to speak of the associations of different individuals as running along certain specified lines, or of showing certain types (*Sachlicher Typus*, *Konstellationstypus*, etc.). However, there exists no very definite knowledge of how consistently such reactional tendencies are preserved, and of how far any given series of observations with a single individual may be indicative of that individual's type of response to the association test.<sup>2</sup> With a view to contributing to a better understanding of these matters there were undertaken the calculations that form the basis of the present report.

The experimental material is largely that discussed from other standpoints elsewhere; it includes a total of 10,900 observations, with 28 different subjects, 11 men and 17 women, hospital nurses with the exception of two physicians. For further account of the experimental conditions the reader may be referred to the previous discussions.<sup>3</sup>

The working concept of individual differences, for the association test and for measurements in general, is broadly based upon the postulate that any given individual preserves, within the function measured, a range of variability that is less than the variability of normal individuals about the

<sup>1</sup> From the McLean Hospital, Waverley, Mass.

<sup>2</sup> The study most nearly related to the present is that of Fürst, *Journal für Psychol. u. Neurol.*, IX., 1907, 243-278.

<sup>3</sup> 'Practise Effects in Free Association.' *American Journal of Psychology*, Vol. XXII., pp. 1-13. 'Some Properties of the Free Association Time.' *PSYCHOLOGICAL REVIEW*, Vol. XXIII., pp. 1-23.

normal average. In the absence of external factors such as practise, individuals must vary from themselves at different times less than they do from each other; and even in the equal presence of such factors, the individual relationships must still be preserved. To the extent to which these conditions are satisfied, we may speak of clear-cut individual differences in our various functions.

The variation of the same individual from time to time may be termed the *personal variation*, as distinguished from his variation from similar measurements of other individuals. The comparison of this *personal variation* with the mean variation of a group affords an everywhere interesting psychological datum. In some previous experiments, it thus appeared that individuals varied relatively much less among themselves and more among each other, in the preference for certain sorts of pictures than in the judgment of color-differences or of weights.<sup>1</sup> Indeed, if the result quoted for the weights were generally valid, the accuracy of weight perception would be of almost negligible value as a factor of individual difference. It is certainly desirable that we should have clear knowledge of the relationship of the personal variation to the group variation in all important psychological measures.

If no constant deviations, such as practise effects, are indicated, it is probably sufficient to know the comparative magnitudes of the mean variations of the function taken personally and for the groups: but where such factors are obviously present, the demands of the situation are much better met by the correlation methods. The three functions of the association experiment that appear for consideration in these respects are (1) the reaction time, (2) the tendency to give common or specialized responses (Kent and Rosanoff), (3) the relational character of the responses with reference to the stimulus word, as given in the quasi-logical methods of classifying the responses (Aschaffenburg, Münsterberg, Wreschner, Jung, et al.).

During the latter part of October, 1909, twenty-five

<sup>1</sup>'On the Variability of Individual Judgment.' Essays in Honor of William James, 511-549.



subjects, ten men and fifteen women, underwent the association experiment of one hundred stimulus words prepared and standardized by Kent and Rosanoff. Fourteen months later, in December, 1910, such of these subjects as were still available, fourteen in number, underwent the experiment a second time. We are concerned here with the comparison of the records made by the different subjects with those made by the same subject on the different occasions.

The median association times for the one hundred associations of each individual in each subject compare as follows in the two experiments. The time unit, here as elsewhere, is one-fifth of a second.

Subj.	Oct., 1909	Dec., 1910
I.....	22.0	7.6
II*.....	14.8	8.0
III.....	8.3	8.2
IV.....	8.8	8.5
V*.....	19.0	15.6
VI.....	8.2	6.1
VII*.....	13.4	7.8
VIII.....	12.8	11.0
IX.....	8.7	7.2
X.....	8.0	6.3
XI.....	7.7	6.6
XII.....	7.8	6.9
XIII.....	10.2	8.3
XIV.....	8.6	7.3

The subjects marked with an asterisk are not included in these immediate calculations because of extensive intervening practise, though it is not absolutely certain that V. should have been excluded on this ground. The record is in other ways an exceptional one.

The enormous difference in the performances of subject I. is probably less important than it looks. Every other experience with the test indicates it to be one of those unfortunate experimental accidents that result from some factor, uncontrolled, because insufficiently appreciated; the second experiment is probably the representative one. It may be remarked in passing that the type of association is little different in the two tests with this subject; indeed, the actual response words were in 47 per cent. of the cases the same as before.

The generally shorter times of the second experiment may and probably should be taken as the product of better adaptation to the test. It is true that there is considerable intervening opportunity for practise in manipulating the stopwatch, but the changes are not such as would be induced in this way. It is not likely that the different medians would be thus affected in so different degrees. Very long times are also much less frequent in the second experiment than in the first. Of those reaction times of 50 (10 seconds) and over in length, the unpractised subjects had 37 in the first experiment, 9 in the second. In the matter of a better experimental adaptation, this fact means considerably more than the slight decrease in the medians.

The uniform shortening of the time limits greatly the significance of the comparative mean variations. For the unpractised subjects, the average time is 10.1 in the first experiment, and 7.6 in the second; while the mean variations of these two averages are 2.8 and 1.0 respectively. The average personal variation between the two experiments is 1.2, if the anomalous first case be included, .64 without it.<sup>1</sup> If the former be compared with the group mean variation for the first experiment, and the latter with the second experiment, it is seen that, in spite of the uniform decrease in time, the personal variations are smaller by about one third.

For the reasons indicated, however, the correlation methods furnish the more logical evidence. It is altogether distorted here by the practised subjects, and especially the anomalous case. If the practised subjects alone be excluded, there is still no correlation worth mentioning, the Pearson approximating .28; but if the anomalous case be also ruled out, the Pearson of the remaining ten figures out at .86. This is surely an extreme instance of distortion by a single anomalous observation, which it is presumably more accurate to exclude. In the remaining cases, the tendency to preserve the type in reaction time is fairly definite.

In calculating the reaction times of the second series of experiments, it was thought worth while to observe how much

<sup>1</sup> With correction for constant error, this quantity becomes .27.

the two halves of the experiment might differ in this respect. Fifty observations should here give a fairly reliable median, and we should be assured of a greater constancy of experimental attitude than in experiments separated by a wide interval. The times have a fairly uniform tendency to run longer in the second half of the experiment; it would be interesting to determine if there were a fatigue phenomenon here, as might perhaps be done with larger series of stimulus words; in this instance it is quite possible that the second fifty of the words chance to be more difficult.

Before leaving this phase of the matter, attention may be called once more to the character of the practise curves reproduced in a previous contribution.<sup>1</sup> In the median of fifty reaction times, the differences between four of the subjects are seen to be entirely definite; the practise curves of RED, BROWN, BLACK and BLUE, never touching except for one intersection of the last two. This evidence supports that of the figures just given, that clear-cut individual differences are to be found in the reaction time of the association experiment. It is less certain that this reaction time is determined in such a way as to make a particularly significant psychological measure. Like a high cephalic index, a long reaction time for an individual may be quite definitely made out, but both may mean absolutely different things according to the setting in which they occur.

It is but natural that in the second test a stimulus word frequently elicits the same response as in the first test. The number of responses thus repeated to the same stimulus word has found some application in pathological cases, the practise being to rate the subject higher, the more responses were changed. This interpretation is scarcely indicated for it here. The figures are as follows:

(Number of responses identical in each experiment)

Subject	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	XIII	XIV
First half. . . . .	24	7	12	21	8	19	0	8	24	22	21	23	22	26
Second half. . . . .	23	18	21	21	10	30	5	14	14	31	25	21	29	28
Total. . . . .	47	25	33	42	18	49	5	22	38	53	46	44	51	54

<sup>1</sup> *American Journal of Psychology*, XXII., plate facing page 2.



The range is quite extensive and a form of distribution is distinct. Excluding the practised subjects, the personal variation in this function compares directly with the group variation in the ratio of about  $5\frac{1}{2}$  to 7; that is, individual differences among these subjects are not very well defined here. There appears, however, some correspondence of the tendency to differentiate the responses *in toto* with a large number of predicate associations, as well as with a tendency to individualized responses.

The simplest criterion of the egocentricity of a Kent-Rosanoff record is the number of 'individual' reactions, that is, as Kent and Rosanoff use the term, the number of given responses that do not occur in their frequency tables (or for a subject that is concerned in these tables, occur only the one time that that subject gave them). Through analyzing large masses of material in this way, it is doubtless possible to reach a number of interesting and useful conclusions. The number of these 'individual' reactions occurring in the first and second experiments is as follows in the two tests:

Subject	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	XIII	XIV
No. ind. react. 1st. exp..	0	9	2	3	4	2	17	1	4	2	2	2	8	0
No. ind. react. 2d exp...	0	5	10	1	15	5	31	9	4	2	1	0	3	3

For all subjects, the averages are 4.0 individual reactions in the first experiment, and 6.3 in the second experiment. The group variation is 3.2 in the first, and 5.5 in the second. The average personal variation between the two experiments is 2.1. The correlation is low, being but 65 per cent. of plus signs from the median. Excluding the practised subjects, the individual differences become even less distinct, the personal variation being as great as the group variation of the first experiment.

The failure of this measure for the present calculations is due entirely to its not being sufficiently fine-drawn. It has the advantage of being rapidly determined, and perhaps suffices for comparisons between considerable groups of subjects; but in comparisons between different individuals, and

especially between different records of the same individual, it is not only much too coarse, but may be actually misleading.

For such purposes, the detailed analysis of the data of the frequency tables amply repays the time that it requires. Each association reaction has a certain value from 0 up to a possible 1,000 and an actual 650, according to its place in the frequency tables.<sup>1</sup> For each record may be calculated a *coefficient of community*, this being the median value of all the associations in that record. This figure varies within wide limits for different individuals. If it is high, it indicates a tendency to respond with common, ordinary reactions; if low, it indicates a tendency towards specialized and individualized responses. This coefficient of community, which may be abbreviated as *c*, is as follows for the different subjects:

Subject	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	XIII	XIV
<i>c</i> , 1st exp. . .	16.5	2.2	9.0	11.5	15.1	8.5	2.2	6.3	6.6	17.4	9.5	15.2	9.3	11.0
<i>c</i> , 2d exp. . . .	15.1	3.0	6.0	10.0	2.0	7.6	.7	3.3	9.0	18.0	12.0	13.5	7.2	9.3

The most cursory glance at these figures shows that there is in reality a high degree of inherency in the tendency to give common or specialized responses, which the mere number of individual reactions altogether fails to indicate. The mean group variations of the first and second experiments are 3.8 and 4.1 respectively. The personal variation between the experiments is 1.3 including the anomalous Case V. There has here been a far-reaching alteration in the form as well as in the individuality of the associations. Without this case the group variations would approximate 3.7 and 3.9, the personal variation being .9. In the correlation methods, the only negative case from the median is that of V.; elsewhere the results with and without it are as follows:

	Incl. V.	Excl. V.
Order displacement.....	18 per cent.	9 per cent.
Pearson.....	.73	.83

In this function therefore, we appear to possess a measure of individual differences that will bear comparison with any

<sup>1</sup> Kent and Rosanoff, 'A Study of Association in Insanity,' *American Journal of Insanity*, Vol. XXVII., pp. 40, 48-96.

other psychological measure of equal complexity and significance.

The median of community has been calculated separately for the responses which are the same in the two experiments and for those which are different. The figures show some points worth bringing out and are as follows:

Subject	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	XIII	XIV
c repeated responses . . . . .	25.7	6.2	14.7	30.6	14.1	18.4	12.5	16.5	16.6	24.6	21.3	21.7	17.2	15.6
c changed responses (1) . .	10.4	1.4	4.6	5.2	15.2	4.6	1.9	4.9	3.6	10.2	3.7	10.0	6.2	4.8
c changed responses (2) . .	7.1	2.4	1.2	4.9	1.4	4.1	.6	2.3	3.8	9.2	6.1	6.2	4.7	4.3

The main trends of this table are of course those of the one for the whole body of responses; but it is interesting to observe the great disparity in the usualness of the repeated and changed responses. The usual responses are those which are well grounded in experience, and naturally have the greatest chance of reappearance. On the other hand, there is a certain irregularity about it; the repeated responses of IV. are the most usual of all, the changed ones are in both experiments less usual than those of I., X. or XII. Rather unusual repeated responses but relatively usual changed responses are given by XIII. and XIV. The anomaly of Case V. is again evident. The correlations are not so well marked in the changed responses as in the series as a whole; the personal variation is 1.3 as against group variations of 2.8 and 1.9. The orders have 29 per cent. of displacement, 20 per cent. excluding V.; there are 80 per cent. of plus signs from the median.

In concluding the immediate discussion of the type in the community of response, it may be remarked that the distribution of the single figures from which the median is calculated uniformly have a distribution markedly skewed toward the 'individual' end, with now and then suggestions of a species at frequencies between 30 and 40 per cent. A few of the records were examined, by per cent. of like signs, with reference to correlations between the increased individuality and long reaction time; a positive relationship appeared,



rather constant, but quite slight. The median of community in all reactions of over 50 (10 seconds) in the first experiment was, however, given as no higher than 3.5 per cent.

Individual differences in community of response, however well defined or clearly significant, are subject to considerable limits in their application. One cannot operate with this measure outside the stimulus words from which frequency tables are compiled. It is certainly most desirable to investigate if there exists a method of attaining a similar insight into free association material that shall not be subject to these limitations. The obvious direction of such an inquiry is towards the superficial relation of the response word to the stimulus word. It has been put forward by previous investigators that certain psychological attitudes<sup>1</sup> are dynamically related to special relationships of this nature in the association test. The present task is then to test the value of this principle in the determination of individual differences in response to the test.

The relationships to be considered have already been described. There are here five categories, (1) the *predicate*, (2) the *supraordinate*, (3) the *contrast*, (4) the *miscellaneous* or 'internal-objective,' and (5) the *speech-habit group*.<sup>2</sup> Every individual's associations fall, in varying proportion, into these five classes. Continuing to deal with the Kent-Rosanoff experiment, our first question, following the previous system, is how the personal variation in these categories compares with the group variation.

It will be remembered that there are two experiments with fourteen subjects, fourteen months apart. The averages, M.V's. between the different subjects, and M.V's. between the same subjects, are as follows:

The figures speak for themselves about as well as any discussion might do. Save only in the speech-habit category, the individual differences are as clear cut as in the objectively determined frequencies of response (it may be

<sup>1</sup> E. g., the "Prädikattypus" of Jung.

<sup>2</sup> PSYCHOLOGICAL REVIEW, Vol. XVIII., pp. 229-233. For further definition, illustration and discussion of these five categories as such, the reader is referred to this paper.

Category	Group Av.		Group M.V.		Personal M.V. 1st and 2d Exps.
	1st Exp.	2d Exp.	1st Exp.	2d Exp.	
Predicate.....	19.5	22.9	13.5	13.8	3.7
Supraordinate.....	15.5	13.5	9.4	12.5	3.21
Contrast.....	14.9	12.1	8.1	7.1	3.2
Miscellaneous.....	44.5	47.6	5.9	6.9	2.2
Speech-habit.....	5.5	3.9	1.6	1.3	1.6

remarked in this connection that some eight months intervened between the classification of the two sets of results). They are quite definite in the predicate, for external reasons the most important, category, and somewhat less so in the supraordinate and contrast groups. The order in the number of predicates shows 9 per cent. of displacement between the two experiments. The Pearsons in the different categories in the two experiments are as follows:

Predicate.....	.83
Supraordinate.....	.77
Contrast.....	.76
Miscellaneous.....	.84
Speech-habit.....	.03

Relative tendency towards one category strongly implies tendency towards it later, save only in the small category of speech-habit responses. Definite fidelity to type appears, then, to be characteristic of the subjects in these functions of the experiment.

While these are the main findings in this respect, there is another feature of the variability that is not without interest. Each subject naturally varies a certain amount from the group in each experiment, and then a certain amount from himself between the two experiments; it is the average of these figures for each category that has just been quoted. It were also a significant question of whether a subject who tended to vary much from the group also tended to preserve greater or less fidelity to his own type. A distinct negative correlation, 14 per cent. of + signs from the median,  $-.40$  Pearson,<sup>1</sup> is indicated between these two functions. This is probably significant mainly for the subjects who lie

<sup>1</sup> As with the median, only two cases are positive, but these pronouncedly so.

at some distance from the group average, and tend to preserve their type. At first glance, it would scarcely seem reasonable to suppose that a subject who was close to the group average, should vary more from himself than one who was far from it; yet the function does seem to possess some properties of this nature, as will be brought out in the observations of a more intensive nature now to be described.

Hitherto, no comparisons of the material with reference to association types have been made between series of different stimulus words. The extent of the error which different series of words might introduce was not known, and it was therefore endeavored to avoid it, though it was reasonably certain that its extent was not very great. A fairly precise idea of it, however, may be attained in comparing the relation of the associative categories in the first half of the Kent-Rosanoff experiment to those in the second half;<sup>1</sup> there thus being two groups of fifty words each. Changes occurring here are presumably due rather to a difference in the stimulus words than to fluctuations of experimental attitude such as might operate in experiments on different days. The following figures, then, give the mean personal variation of the five categories for the first and second halves for both experiments:

	Pred.	Supra.	Cont.	Misc.	Sp. H.
1st exp. ....	1.2	1.3	2.0	1.8	1.4
2d exp. ....	2.0	1.8	1.4	2.8	.6

The figures of the personal variation between the two halves of the same experiment are somewhat, not greatly, smaller than between the two experiments themselves; though considering that it is for only half the cases, they can scarcely be counted smaller at all. It is peculiar that the second experiment should show greater variability in this respect than in the first; the contrary if anything would be expected.

The indication, however, is that the variation between the different stimulus words would not be sufficient to obscure

<sup>1</sup> A better procedure would have been to compare alternate words.



real individual differences in association type, even though the stimulus words furnishing the material were not the same for each subject.

This question is more searchingly tested in the subsequent analysis of the 7,000 reactions which form the material of the already reported practice experiments. While this material is twice as extensive as that from the Kent-Rosanoff experiment, its interpretation with reference to the matter of types depends largely on whether or not the daily changing of the stimulus words induces special changes in associative trend peculiar to the series that is used. In any case, whatever alteration of type the changes in the stimulus words produces it ought to be in the same direction for each of the seven subjects, since the changes in the stimulus words are the same for each one; that is, if an association series did exist which prejudiced in favor of the egocentric or other categories of reaction, there ought to be a uniform change in that direction among the subjects; indeed, that would be our only criterion of such a change. It has actually been found that on nearly half the days when the egocentric category shows deviations greater than the M.V., such greater variations are in different directions for different subjects. While some influence probably is exercised by particular series of words, it is not believed sufficient, in view of such like findings, to offset the validity of the subsequent interpretations.

*A priori* such an influence would here be quite improbable, since the order of 1,000 words is a quite random selection.

According to observed deficiencies in the conception of the predicate category obtaining in the above it is here widened and the name changed to the *egocentric*. This widening is mainly at the expense of the miscellaneous and speech-habit categories, and consists chiefly in the transference to the former predicate category of reactions where a proper name is involved. This adds much to the significance of the classification.

As there are 1,000 reactions for six, and 500 for two subjects, it is possible to consider the prevalence of special categories in the direct comparison of the various subjects.

The averages and mean variations of each of the eight subjects in the five categories are as follows:<sup>1</sup>

Subject	Black	Brown	Red	Orange	Green	Blue	VII	VIII
Egocentric.....	8.5	7.6	8.7	21.3	22.1	10.7	8.7	16.7
M.V.....	3.2	2.6	2.8	3.2	2.6	3.6	3.5	1.7
Supraordinate.....	1.4	3.3	6.1	2.5	5.0	3.4	6.8	1.7
M.V.....	1.0	1.7	3.2	2.2	2.0	1.8	2.1	.9
Contrast.....	5.0	5.1	3.1	.9	.5	1.9	4.0	.6
M.V.....	2.1	2.4	1.8	1.1	.5	2.1	2.0	.6
Miscellaneous.....	27.7	28.3	28.8	20.3	20.2	26.9	26.3	28.0
M.V.....	4.2	2.6	4.2	3.3	2.5	3.8	2.8	2.6
Speech-habit.....	6.8	5.2	3.0	5.0	2.0	5.2	3.9	3.0
M.V.....	1.9	2.1	1.2	3.4	1.5	1.7	1.3	1.6

It appears here, as also in the previous analysis, that while there is an influence of practise on the form of association, it does not have any special influence on the type, save only in subject BLUE.

In the common acceptance of the experiment, the essential significance lies in the relations of the egocentric and the miscellaneous categories, and more especially in the character of the egocentric category. It is very evident that in this respect three steps are represented in the results: (1) those showing the fewest egocentric reactions, namely, BLACK, BROWN, RED, BLUE and VII.; (2) subject VIII., who occupies a midway position; and (3) ORANGE and GREEN, who occupy positions quite high in the scale of egocentricity, not only here but in the frequency valuations of the Kent-Rosanoff experiment where they are, respectively, subjects VII. and II. These three steps are separated from each other well beyond the limits of the probable error; save in one case only, beyond that of the mean variation. The variability of the measures in each subject is so small compared to the total range in the egocentric category, that there can be little doubt that the averages represent definite points of the scale about which each subject's results center with comparatively little chance fluctuation. On the basis of

<sup>1</sup> These are the actual figures from series of fifty stimulus words; they must be multiplied by two for direct comparison with tables on page .

these figures then, we may assign definite and limited positions to the different subjects along the scale of egocentricity in response. In the other categories, individual difference is much less pronounced. One may note the relative abundance of supraordinate reactions in VII., and their absence in BLACK and VIII.; also the peculiar absence of contrast reactions in ORANGE, GREEN and VIII. The excess of egocentric reactions in ORANGE and GREEN is associated with a corresponding decrease in the miscellaneous category. Elsewhere, little individual difference exists between these subjects, the comparative table of personal and group variations being:

	Egoc.	Supra.	Cont.	Misc.	Sp. H.
Average group var.....	5.1	1.9	1.7	2.7	1.3
Average personal var.....	2.9	1.9	1.5	3.3	1.8

The main interest of further calculation attaches to the possible correlations of the various categories. Thus there is a pronounced negative correlation, approximating  $-.65$  Pearson between the supraordinates of the first Kent-Rosanoff experiment with twenty-five subjects, and the contrast associations of this group. A somewhat less marked negative relationship,  $-.45$ , exists between the contrasts and the predicates in this experiment. In the table just quoted, this is markedly changed. The *egocentric* category, which is supposed to represent more accurately the same psychological attitude that the *predicate* implies, shows an extreme negative correlation with the contrasts,  $-.95$ . On the other hand, the supraordinates are so much reduced by practise effect that the initial negative relationship with the contrast has disappeared, and given place to a slightly positive one of  $.14$ . The results are not inconsistent when one takes into account the practise effect on the supraordinates and the changed conception of the egocentric. A possible interpretation is that the mental mechanism of the contrast association is throughout in marked opposition to that of the predicate or egocentric. At the beginning of practise this is true also of the supraordinates, which here result largely



from the adoption of a conscious 'set' and are thus egocentrically determined; the disappearance of the supraordinates with practise coincides with the increasing freedom of response, and diminished necessity for conscious 'sets.'

A previous paragraph on the Kent-Rosanoff material brought out the result that there were marked individual differences in fidelity to type, and that those tended to be more faithful to their own types who were further away from the group averages. The practise experiments throw the essential features of this result into stronger relief. The 'average' association type appears to be a distinctly concrete one. A fair average association record of fifty words would contain about 10 predicates, 7 supraordinates, 8 contrasts, 22 miscellaneous and 3 speech-habit. The great departures from this type are in the egocentric direction; and the above finding may be re-stated in terms of a greater fidelity to type among individuals of markedly egocentric reaction tendency. It will be seen that the number of egocentric reactions is not so variable in ORANGE, GREEN and VIII., who represent egocentric types, as it is in BLACK, BROWN, RED, BLUE and VII., who represent concrete types.

The relatively large M.V. of the egocentric category in ORANGE is due largely to three days, on which there was an extraordinary variation from type; the median variation would be 2.6. Larger single variations from type in this category occur among the egocentric subjects than among the concrete; they tend to keep closer to type, but may be thrown farther away from it.

It will be distinctly understood that by an association type is meant the tendency of an individual to preserve a definite associational trend and this only; nothing has been adduced to show whether there are distinct *species* of 'egocentric' 'concrete,' or perhaps of other types. The number of cases is scarcely sufficient for a final determination of whether species exist, but they are not indicated in the examination that it has been possible to make. The question of species is probably to be cleared up, however, by a study to this end of the material gathered by Kent and Rosanoff, *in extenso*.

From actual observation, as well as from what we know of the psychology of the predicate reaction, there should be every reason to expect definite negative correlation between the tendency to give the common responses of the Kent-Rosanoff tables and the tendency towards an egocentric reaction type as given in the number of predicate responses. One can indeed see that 'unusual,' and 'egocentric' reactions are the product of largely the same mental mechanisms. The actual figures of this correlation are:

MEDIAN OF COMMUNITY AND NUMBER OF PREDICATES

1st exp.....	-.74
2d exp.....	-.74

This material was classified before the conception of the egocentric category had replaced that of the predicate, it was this very classification that indicated the desirability of the 'egocentric' conception. The *r*'s would be materially higher if the 'egocentric' category had been employed, for it would have included a fairly numerous group almost always summed up in the 'individual' reactions, the proper names.

The figures of the correlation are the empirical measure of the validity of this conception. The object has been to define a category which shall be egocentric in fact as well as in name, and these coefficients plus the considerable margin of proper names, give the extent to which this object has been reached.

The results which appear more significant for further progress with the test may be summarized as follows:

1. The previously reported differences in the association time of various subjects are fairly inherent; that is, a certain range of reaction time seems characteristic of a given individual. A long median reaction time is usually associated with a skew distribution much spread out at the long end. Such an abundance of very long reaction times indicates the presence of special difficulties in responding. These may be of widely differing causations, the determination of which demands further analysis of the record.

2. The number of individual reactions appears hardly suitable for the comparison of single records and the median

value of the 100 reactions is substituted. In the tendency to give common or specialized responses by the Kent-Rosanoff experiment, individual differences are much more clear cut than in the association time. The extreme range of individual difference in single records is about 20 : 1 as against 4 : 1 in the association time. The responses (of the same subject to the same stimulus word) which differ in the two experiments, are in general less than one third as 'usual' responses as those which are the same on each occasion.

3. In this same material of the Kent-Rosanoff experiment, definite fidelity to type appears in the tendencies to give predicate, supraordinate, contrast and 'internal-objective' responses, but not in speech-habit reactions. A relatively greater fidelity to type is found among individuals whose reaction type is further distant from the central tendency of the group.

4. When the number of stimulus words in a chance-selected series is as great as fifty, the results of different series of stimulus words are for practical purposes comparable with each other in respect to the above classification by logical category. A study in eight subjects, through extended series of stimulus words, illustrated in greater detail the distinction between the individual of the *egocentric*, as apart from the individual with the *concrete* association type. Of the eight subjects, two were pronouncedly egocentric, one rather so, and five of the more concrete type.

5. There is, initially, marked negative correlation of the contrast category on the one hand, with the predicate and supraordinate categories on the other. Practise breaks down this correlation for the supraordinates, but preserves it for the mechanism represented by the predicates. Pronounced negative correlation is indicated between the egocentric (resp. predicate) category and the community of response; that is, 'egocentric' responses are seen to be unusual in the Kent-Rosanoff tables, as one would expect.

There is little significant correlation between the usualness of the response and its reaction time, as was also observed by Kent and Rosanoff; it is possible that a more definite, though



rather complex, relationship obtains in this respect between different individuals or groups. The fidelity to association type has also been largely stated in terms of correlation measures, which range between 73 and 86 per cent. positive.

It has thus been attempted to indicate the character of the variation in the principal functions of the association test; the causes of these variations, under what conditions they occur, and their relations to the personality, are to be discussed elsewhere. The present result is that the findings of quantitative study have not contradicted the data of ordinary observation, and give an affirmative answer to the question of association types.

# EXPERIMENTAL STUDIES OF RHYTHM AND TIME

BY J. E. WALLACE WALLIN

## III.<sup>1</sup> THE ESTIMATION OF THE MID-RATE BETWEEN TWO TEMPOS

### A. EXPLANATION OF PROBLEM

The problem of this investigation consists in the selection or production of a tempo lying midway between two standard tempos produced by a Verdin metronome. For example: suppose the subject is made to listen to the 40 (40 per minute) rate, and then, after a brief interval, to the 208 rate. What is the mid-tempo between these limiting stimuli? How accurately can the subjects make a mental estimation of the mid-rate? The problem, of course, does not concern the mathematical computation of the mid-rate—none of the subjects knew the rate of the standards<sup>2</sup> employed—but the selection from among a number of variables of the rate which appears to lie midway between the standards, or the production by tapping of the mid-tempo.

Three pairs of standards were employed in the following combinations: 40-208; 208-40; 72-176; 176-72; 104-144; and 144-104. The mathematical mean is the same for all three pairs, namely 124 beats per minute, or 2.066 beats per second. Accordingly the length of the mid-interval (the time from click to click) is .483 sec.<sup>3</sup>

<sup>1</sup> Articles I. and II. appeared in the March and May numbers, 1911, of the *PSYCHOLOGICAL REVIEW*.

<sup>2</sup> The extreme or limiting rates will be referred to as the standards or rates.

<sup>3</sup> The computation of the arithmetical mean is based on the assumption that if the difference between  $r_2$  and  $r_1$  is equal to the difference between  $r_1$  and  $r$  (see accompanying diagram), then  $r_1$  = the arithmetical mean between  $r$  and  $r_2$ . On the other hand, if the relative sensible discrimination is constant the difference between  $r_2$  and  $r_1$  will be greater than the difference between  $r_1$  and  $r$  when the two seem equal to sensation, whence the proportion  $r : r_1 = r : r_2$ , or  $r_1 = \sqrt{r \cdot r_2}$ . This gives the geometrical mean, which amounts to the following for the different pairs: 91.2 for 40-208; 112.6 for 72-176; and 122.3 for 104-144. (The corresponding geometrical means in interval length are .65 sec., .53 sec. and .49 sec.)

## RATES PER MINUTE

$r_2$	$r_1$	$r$
Faster standard	Mid-rate	Slower standard
208		40
176	124	72
144		104

$(208 + 40 = 248 \div 2 = 124. \quad 208 - 40 = 168 \div 2 = 84. \quad 84 + 40 = 124.$   
 $208 - 84 = 124.)$

The attempt was made to vary regularly the time order in which the different standards and the different pairs of combinations were given to the different subjects, in order to neutralize any constant errors in the final averages. Thus to some the 40-208 pair was given first; to others the 72-176, and to others the 104-144 pair. Likewise to some subjects the faster of the two standards was presented first, and to others the slower. Thus some listened first to the combination 208-40, and then, after an interval of four or five seconds, to the reverse order, 40-208; while for the others the order was, first, slow-fast (40-208), and second, fast-slow<sup>1</sup> (208-40).

The precise order is given in detail for each subject in the columns in the tables headed 'Time Order.' Thus, to subject L., Tables VI. to IX., the first combination presented was 40-208 (indicated by I. in the second column); then followed in order 208-40 (II.), 176-72, 72-176, 144-104 and finally 104-144. In all cases the results are averaged separately for the rapid-slow and slow-rapid combinations of standards. Thus Table I. averages the results for 40-208, and Table II. for 208-40. The last average in II. is the average for both tables. In Tables VII. to IX. not only are the separate and combined averages given in the same table, but the results are arranged in subordinate groups according as the rapid-slow combination was given *before* or *after* the slow-rapid. This order is indicated by the letters *a* and *b* in the third column of these tables, *a* indicating that the given combination of standards was given first and *b* that it was given second. Thus in Table VII. the order 40-208 was given before the order 208-40 to J, H and D, while 208-40 was given before 40-208 to W, S, R, B and Ms.

Invariably the two tests for the same pair of standards were finished before either of the other two pairs were presented. Thus 40-208 was always directly followed by 208-40 (or vice versa), and never by one of the other two pairs.

The first experiment was conducted in the Princeton Psychological Laboratory during February, March and April, and the second during May and June in 1906—the publication has been delayed for reasons given in the first article. The subjects consisted of seniors, all of whom had taken two terms of work in the laboratory, and Professors E. G. Spaulding, W. T. Marvin, M. P. Mason, A. L. Jones

<sup>1</sup> In discussing the various pairs the slower standard will be mentioned first (40-208, 72-176, 104-144). But, unless otherwise indicated by the context, the facts stated are based on the averages for both the slow-fast and fast-slow series.



and H. C. Warren. The latter two and three of the students (D, H, R) served in both experiments.

The subjects invariably sat with their backs towards the apparatus, hence could not see the manipulation of the metronome, and were given no information other than that involved in the explanation of the problem.

### B. FIRST EXPERIMENT<sup>1</sup>

#### ESTIMATION OF THE MID-RATE BETWEEN TWO METRONOME TEMPOS, BY THE METHOD OF CHOOSING A VARIABLE IN A SERIAL PROCEDURE

TABLE I

*Standards: 40 and 208 (beats per minute)*

Starting with Variable too Slow

S.	Time Order	Variable Started at	Beats per Minute	M.V.
L.....	II.	58	92	
K.....	III.	63	116	
H.....	II.	69	92	
R.....	IX.	76	104	
Wa.....	XII.	69	126	
D.....	XII.	66	88	
A.....	V.	60	84	
J.....	VIII.	80	116	
G.....	VI.	100	112	
We.....	I.	69	112	
Ms.....	III.	72	132	
I.....	IV.	88	116	
Average <sup>1</sup> .....			107.5	12.9

Starting with Variable too Fast

L.....	I.	160	88	
K.....	IV.	138	108	
H.....	I.	138	126	
R.....	X.	168	132	
Wa.....	XI.	138	120	
D.....	XI.	138	116	
A.....	VI.	138	124	
J.....	VII.	160	138	
G.....	V.	138	120	
We.....	II.	138	120	
Ms.....	IV.	152	126	
I.....	III.	138	120	
Average <sup>1</sup> .....			119.8	7.9
Average <sup>2</sup> .....			113.6	10.4

Explanations follow Table VI.

<sup>1</sup> Being beyond the reach of reference books at the time of compilation I am obliged to lay out the data purely empirically. Moreover, other lines of work now so engross my attention that no other course is possible.

TABLE II

*Standards: 208 and 40*

Starting with Variable too Slow

S.	Time Order	Variable Started at	Beats per Minute	M.V.
L.....	IV.	58	92	
K.....	I.	63	100	
H.....	III.	60	92	
R.....	XII.	76	96	
Wa.....	X.	56	80	
D.....	IX.	60	82	
A.....	VIII.	72	84	
J.....	V.	48	66	
G.....	VII.	54	76	
We.....	IV.	100	112	
Ms.....	II.	66	76	
I.....	I.	69	120	
Average .....			89.6	12.3

Starting with Variable too Fast

L.....	III.	160	96	
K.....	II.	144	124	
H.....	IV.	138	116	
R.....	XI.	160	120	
Wa.....	IX.	138	96	
D.....	X.	138	126	
A.....	VII.	132	108	
J.....	VI.	138	104	
G.....	VIII.	132	112	
We.....	III.	152	120	
Ms.....	I.	144	88	
I.....	II.	138	124	
Average <sup>1</sup> .....			111.1	10.6
Average <sup>2</sup> .....			100.4	11.4
Average <sup>3</sup> .....			107.0	10.9

Explanations follow Table VI.

TABLE III

*Standards: 72 and 176*

Starting with Variable too Slow

S.	Time Order	Variable Started at	Beats per Minute	M.V.
L.....	V.	88	112	
K.....	VII.	96	120	
H.....	VIII.	96	126	
R.....	II.	92	108	
Wa.....	II.	96	116	
D.....	III.	100	116	
A.....	XII.	108	116	
J.....	IX.	100	116	
We.....	VIII.	108	112	
Ms.....	V.	96	112	
I.....	V.	108	126	
Average <sup>1</sup> .....			116.3	4.1

## Starting with Variable too Fast

L.....	VI.	152	112	
K.....	VIII.	138	120	
H.....	VII.	144	126	
R.....	I.	138	124	
Wa.....	I.	144	122	
D.....	IV.	152	132	
A.....	XI.	132	116	
J.....	X.	138	126	
We.....	VII.	132	124	
Ms.....	VI.	132	124	
I.....	VI.	132	126	
Average <sup>1</sup> .....			123.0	4.0
Average <sup>2</sup> .....			119.6	4.0

Explanations follow Table VI.

TABLE IV

Standards: 176 and 72

## Starting with Variable too Slow

S.	Time Order	Variable Started at	Beats per Minute	M.V.
L.....	VII.	88	102	
K.....	VI.	92	120	
H.....	VI.	92	116	
R.....	III.	92	112	
Wa.....	IV.	104	116	
D.....	II.	96	108	
A.....	X.	100	112	
J.....	XI.	104	114	
We.....	VI.	104	116	
Ms.....	VII.	80	104	
I.....	VIII.	112	120	
Average <sup>1</sup> .....			112.7	4.6

## Starting with Variable too Fast

L.....	VIII.	152	112	
K.....	V.	138	120	
H.....	V.	138	126	
R.....	IV.	138	124	
Wa.....	III.	138	116	
D.....	I.	144	126	
A.....	X.	138	116	
J.....	XII.	138	126	
We.....	V.	132	124	
Ms.....	VIII.	132	126	
I.....	VII.	138	126	
Average <sup>1</sup> .....			122.0	4.3
Average <sup>2</sup> .....			117.3	4.4
Average <sup>3</sup> .....			118.4	4.2

Explanations follow Table VI.



TABLE V

*Standards: 104 and 144*  
Starting with Variable too Slow

S.	Time Order	Variable Started at	Beats per Minute	M.V.
L. ....	XI.	108	126	
K. ....	X.	108	124	
H. ....	IX.	108	124	
R. ....	VII.	112	120	
Wa. ....	VII.	112	122	
D. ....	VI.	108	120	
A. ....	III.	112	120	
J. ....	II.	112	120	
G. ....	IV.	112	120	
We. ....	IX.	112	120	
Ms. ....	XI.	116	124	
I. ....	X.	112	120	
Average <sup>1</sup> . . . . .			121.6	1.9

Starting with Variable too Fast

L. ....	XII.	138	120	
K. ....	IX.	132	120	
H. ....	X.	138	124	
R. ....	VIII.	138	118	
Wa. ....	VIII.	132	126	
D. ....	V.	138	126	
A. ....	IV.	138	126	
J. ....	I.	138	120	
G. ....	III.	132	116	
We. ....	X.	138	126	
Ms. ....	XII.	138	126	
I. ....	IX.	132	120	
Average <sup>1</sup> . . . . .			122.3	3.3
Average <sup>2</sup> . . . . .			121.9	2.6

TABLE VI

*Standards: 144 and 104*  
Starting with Variable too Slow

S.	Time Order	Variable Started at	Beats per Minute	M.V.
L. ....	X.	108	124	
K. ....	XI.	108	120	
H. ....	XII.	112	124	
R. ....	V.	108	124	
Wa. ....	VI.	108	122	
D. ....	VIII.	112	120	
A. ....	II.	108	124	
J. ....	III.	108	120	
G. ....	I.	108	124	
We. ....	XII.	116	120	
Ms. ....	IX.	112	122	
I. ....	XII.	112	120	
Average <sup>1</sup> . . . . .			122.0	1.6

## Starting with Variable too Fast

L.....	IX.	138	120	
K.....	XII.	132	116	
H.....	XI.	138	124	
R.....	VI.	132	120	
Wa.....	V.	138	120	
D.....	VII.	132	124	
A.....	I.	138	124	
J.....	IV.	132	117	
G.....	II.	138	122	
We.....	XI.	132	126	
Ms.....	X.	132	124	
I.....	XI.	138	126	
Average <sup>1</sup> .....			122.0	2.7
Average <sup>2</sup> .....			122.0	2.1
Average <sup>3</sup> .....			121.9	2.3

*Explanation of Tables.*—Unit of measurement: the number of clicks made by the metronome per minute. S.: subject (all males). Time order: order of the temporal succession in which the various combinations were given, from I. (first) to XII. (last) M.V.: mean variation between the counts given in fourth column of the tables. Ave.<sup>1</sup>: separate averages for the group of measurements in which the variable started either too slow or too fast. Ave.<sup>2</sup>: separate average for the slow-fast or fast-slow combinations of a given pair, the average of the two Ave.<sup>1</sup>'s. Ave.<sup>3</sup>: general average for a given pair, the average of the Ave.<sup>2</sup>'s. The standards were given in the order indicated in each table. Thus 40-208 indicates that the 40 rate (40 beats per minute) was given before the 208.

*The Selection of the Mid-rate in a Serial Procedure with Variable Rates Introduced between the Limiting Stimuli* (Method of Impression, or Method of Mean Gradations).

The instructions given to the subjects were substantially as follows: "I shall have the metronome click off a series of beats which we shall call the *first standard*. Then I shall click off a series of slower beats (or faster in case the first series consisted of the slow standard) which we shall call the *variable* series or beats.<sup>1</sup> Finally I shall click off a series at a still slower rate of speed which we shall refer to as the *second standard*. I want you to listen carefully (closing your eyes, if necessary) to these three series and tell me, after all three have been given, whether the speed of the variable beats seemed to be nearer the speed in the first standard, or nearer the rate in the second standard." (Or, 'whether the variable

<sup>1</sup> In a few preliminary trials the variable was sometimes given as the second series, and sometimes as the third. Some subjects preferred the first arrangement and some the other.

was too slow or too fast'; or 'whether the length of the intervals in the variable series was midway between the length of the intervals in the first and the second standards.') If the subjects responded correctly that the variable beats were too fast it was explained that in the next test the speed of the variables would be reduced and that this process of minimal approachments would be continued until the variable was judged midway between the extremes. After the equality judgment had been rendered the same step was repeated once or twice, and in case the judgment was reversed the steps were continued until an equality judgment was again obtained. (All except 'I' changed their equality judgments in an average of 2.1 of the twelve series.) After a satisfactory equality judgment had been obtained, as above, the step-procedure was continued in the same direction for 10 of the subjects until the variable was again judged unequal (too slow in the above case). (The equality range averaged 9.1 beats for the 40-208 pair, 4.4 beats for 76.176 and 3.0 beats for the 104-144.) The mid-tempos recorded in the tables are invariably the *first equality judgments* rendered which were not later changed.

After the mid-rate had been thus selected, with the variable starting too fast, the reverse procedure was followed (variable too *slow*, minimal changes). If an equality judgment was given at the very start the variable was started still slower.

It is thus evident that the subjects knew the direction of the progressive changes in the variables series. It was explained, however, that the steps might vary somewhat, and that they would not always start at the same rate of speed in the different combinations. It was necessary to make this precautionary statement in view of criticisms raised against the serial step procedure,<sup>1</sup> and in order to counteract the possible tendency of subjects to count the number of steps employed in a given combination and utilize the knowledge of the number of changes as a guide for judging

<sup>1</sup> Angell states that he was able to get any midpoint of sound intensities he wanted by the serial method, the midpoint varying with the initial value of the variable and with the size of the steps.



later combinations.<sup>1</sup> The rate at which the variable was started for any subject can be determined by consulting column three in Tables I to VI. The steps usually employed were those provided on the pendulum of the metronome, but near the estimated mid-point they were sometimes halved, as explained in the second article. They were always halved when one of the adjacent speeds was judged too fast and the other too slow.

In each of the three series (first standard, variable and second standard) the metronome was allowed to click about ten times—sometimes a little less, sometimes a little more. The one series followed the other as rapidly as the weight on the pendulum could be properly adjusted—an interval of three or four seconds. The number of clicks in each series and the length of the silence between the series seemed to be satisfactory to all the subjects, except one who sometimes found the pause too long (Ms), another who sometimes found it slightly too short (I), and two who found the sound series at times too long (Ms and L, the latter referring to the 40 rate).

The sliding of the weight on the pendulum produced a slight noise, of longer or shorter duration according to the extent of the displacement. Eight who noticed the noise reported that it was not utilized as a secondary criterion for estimating the mid-point, although two found that it distracted the attention and that they had to use deliberate effort to disregard it (Wa, D).

Two or three sittings (three for A, D, I, J, Wa, We) were required to complete the experiment. During the first sitting only one pair of standards was used. Likewise one pair was used in each of the second and third sittings for those who sat three times. The sittings varied from 40 minutes to one hour.

Eight subjects found that the experiment was more or less fatiguing, particularly toward the end of the sitting, and most frequently in connection with the 40-208 pair. One, in order to 'keep the rates alive,' had to resort to nodding the head. Contrariwise, eight did not find the work very fatiguing, particularly with the 104-144 pair. The standards farthest apart accordingly occasioned the greatest strain on the attention.

Of six subjects interrogated all considered the problem considerably more difficult than the parallel problem of choosing a line equally different from a short and a long line. Likewise of four questioned all found the experiment harder than an earlier experiment (to be described in the next article) on determining the maximum number of categories into which the metronome series of auditory rates could be grouped. The difficulty most frequently experienced was to conceive, imagine or know where to locate the mid-rate (mentioned by seven). Five were able to 'ideate' the mid-rate,

<sup>1</sup> Only one, however, reported that he tried to use this expedient once (I).

or to carry in mind a temporal standard (ideal beats or ticks) corresponding to the mid-rate, one obtained a mental criterion after a few trials, and one felt that his criterion changed in the pair with the slowest standard. Five subjects, on the other hand, did not use any mental standard, or any fixed mental beats. Another frequent difficulty was that of keeping the standards in mind (by six)—more difficult with the first standard (by five) than with the second standard (one), or the variable (one). Agreeably with these judgments, the first standard required more attention (reported by five) than the last. Two gave more attention to the variable; one, to the two standards than the variable; one, to the first standard and the variable than to the second standard; one, equal attention to the different series; and one gave attention mainly to the 'contrast' between the rates. Two paid most attention to the slow standard, and one to the fast. The slower standard was retained in the memory more easily than the fast (by six as against two subjects). Although the introspections of the subjects vary more or less from time to time or series to series,<sup>1</sup> it is patent that the *first* standard and the *fast* standard are the most difficult to remember, and would for this reason necessarily require more attention.

The standards *nearest together* (104-144) were felt to be the most *difficult*, by six, (because of the small contrast between them, or the difficulty of distinguishing the grades of difference, or retaining the standards); and the standards *farthest apart* (40-208), by five (because one rate was too fast and the other too slow, or the slow could not be grasped as a unity, or the equating became uncertain, or the variable could not be compared with the slow rate). Three regarded the middle standards (72-176) as the *easiest*, two the nearest together, one the farthest apart; while one considered the difficulty about the same for all three pairs, and one for the two less widely separated pairs. Altogether, introspectively the difficulty is the greatest for the two pairs which are the nearest and the farthest apart.

Less frequently enumerated difficulties encountered in the experiment were the acquisition of a 'habit speed' for the mid-rate in one combination which influenced the selection in other combinations; the reversing of the order of giving the standards, which caused the subject to lose his 'mental mid-point'; the occasional irregularity of the 208 beats; and the lack of a good ear for time.

*Results.*—1. The selected mid-rates are invariably *less (slower) than the arithmetical mean*. The error amounts to 2.1 beats per minute for the 104-144 pair, 5.5 beats for the 72-176 pair, and 17 beats for the 40-208 pair. For the first two pairs the error is practically negligible; the average estimations are very accurate.

It is observable that the estimated mid-rates are slightly nearer the geometrical than the arithmetical mean for the two pairs 40-208 (the divergence from the geometrical mean being 15.8 beats as against a divergence of 17 beats from the arithmetical mean) and 104-144 (0.4 as against 2.1), but

<sup>1</sup> This is true for many of the subjects on most of the questions introspected. The discrepancies in the reports are due to the differences between the standards, differences dependent upon the time order, and other undetermined causes.

the reverse is the case with the 72-176 pair (5.8 as against 5.6).

2. Both the absolute and relative *inaccuracy of estimation increases with the increase in the distance between the limiting stimuli*. The absolute error for the 72-176 pair is two and one half times as large, and for the 40-208 pair eight times as large, as for the 104-144 pair. The relative errors—that is, the amount of error expressed as a per cent. of the distance between the limiting stimuli or standards—amount to 5.0 per cent. for the 104-144, 5.3 per cent. for the 72-176, and 10.1 per cent. for the 40-208 pair. Thus the relative errors for the two pairs in which the standards are the closest is about the same, while the relative error for 40-208 is twice as large as for either of the other two pairs.

These results are confirmed by the absolute M.V.'s (M.V. column), which amount to 10.9 beats per minute for 40-208, 4.2 beats for 72-176, and 2.3 beats for 104-144. Relative to the distance between the limiting stimuli, however, the corresponding mean variations (relative M.V.'s) amount to 6 per cent., 4 per cent. and 5.7 per cent., the relative M.V. thus being least for the 72-176 pair.

Likewise, the absolute range of equality (the range within which equality judgments were rendered), which furnishes an indirect measure of uncertainty, is the smallest for 104-144 and the largest for 40-208, but relative to the distance between the standards the range is smallest for 72-176 (4.2 per cent.), followed by 40-208 (5.4 per cent.) and 104-144 (7 per cent.).

These experimental demonstrations, particularly that the 40-208 pair is the most inaccurate, are supported by the introspections. The 40-208 pair occasioned the greatest amount of *fatigue* and the 104-144 the least; six subjects reported that their estimations were *uncertain* with the 40-208 pair, but only two with the 104-144 pair, and two with the 72-176 pair; while the estimations were considered *most* certain with 40-208 by one, and with 72-176 and 104-144 each by four subjects (different subjects, of course, in each pair), and intermediate for 72-176 and also 104-144 by four. One regarded the latter two sets as about equal. All felt *least* certain with the 40-208 pair.<sup>1</sup>

3. *Giving the slow standards first* enabled the subjects to make *more accurate* estimations than giving the fast standards first, although the differences are small, amounting to 6.6 beats for 40-208, 1.1 for 72-176, and 0.1 for 104-144 (but in the last the accuracy was greater with the fast standard first), or an average of 2.6 beats for the three standards.

<sup>1</sup> Why more subjects should regard the pair of standards nearest together as more 'difficult' than the pair farthest apart, as we saw earlier, although the certainty of the equating was the least for the latter, is not clear. The two judgments appear to be contradictory.



This conclusion gains slight support from the *introspections*. Six preferred to have the *slow* standard come first (because it was easier retained, or the decisions could be rendered faster, or the comparison was easier, or it seemed like the natural order); while five preferred the *faster* standard first (because it was easier retained, or because, although harder to retain, it could be kept more easily with the finger, or because the decisions could be made faster, since the slow standard was 'fixed in the mind.' Six, however, at one time or another, felt that the order was immaterial. The only effect material to one was the feeling of uncertainty occasioned by *reversing* the order of the standards.

4. The estimations are rendered *more accurately* when the *variable is started too fast* rather than too slow (slower than the mid-tempo, of course). The differences in beats per minute are as follows for the various combinations: 12.3 for 40-208; 10.8 for 208-40; 3.3 for 72-176; 4.6 for 176-72; .3 for 104-144; and .00 for 104-144. The average difference for all combinations amounts to 5.2 beats.

The conclusion is confirmed by the M.V's, which are smaller in all the combinations in which the variable was started too fast except two (104-144 and 144-104). The average difference for all combinations amounts to .8 beat.

On this point, however, the *introspections* are neutral. Five preferred to have the variable start *too fast* (because it was easier to retain, or the mid-point could be reached faster, or since the slower standard was easily retained it was only necessary to compare the two faster rates, which was easily done since they came nearer together); five, at one time or another, preferred to have it start *too slow* (because easier to retain, or the judgments were more certain, or it was easier to compare the two slower series of rates); while six expressed no preference (twice for 40-208, thrice for 72-176, and five times for 104-144, so that the feeling of neutrality mostly attached to the standards closest together). There is a tendency to express preference when the standards are rather far apart.

5. The *degree of accuracy* with which different subjects are able to *determine the mid-point* between different limiting rates of temporal successions differs considerably. This has been indicated by the M.V's, and may be seen from a statement of the largest and smallest errors made in each pair. The largest errors in the 40-208 pair was 66 beats; in the 72-176, 22 beats; and in the 104-144 8 beats. Contrariwise, there were three instances in which the estimations were absolutely correct in the first of the above pairs, five in the second, and thirteen in the third.

This experimental difference is paralleled by a corresponding subjective difference in certainty. Nine in one combination or another felt uncertain, or rated their estimations as guesses, particularly near the point of equality; four, in one combination or another, rated them as better than guesses; while eight felt certain or fairly certain.

in one combination or another. One felt more certain of his 'just different' estimations than his equality judgments. The subjective certainty, as we saw above, was least for 40-208.

Since the subjective certainty varied more or less in the different combinations, the question naturally arises whether the feeling of certainty is paralleled by a corresponding superiority in the experimental records. In the case of nine subjects the records do (some strongly, others only partly), while in the case of four they do not, corroborate, or only corroborate in some combination, the subjective evaluations. Thus 'Wa' was decidedly less certain in the 40-208 pair, but his record was poor only in the 208-40 combination. 'A' felt more certain in the order 104-144 than with 144-104, but he was more accurate in the latter combination. While the experimental differences were usually *less* than the subjective differences would have led us to expect, in the main the subjective evaluations were quite reliable.

6. How were the subjects able to estimate the mid-rates? Did they base the estimations on the '*immediate impressions*' received from the stimuli, or on *secondary factors*? Were their judgments of the immediate or the reflective type? If secondary factors had to be used to aid the judgments, what was their nature?

Six believed that they based their decisions in one combination or another on the *absolute impressions* received from the stimuli, on a 'sensation of difference,' or on the 'time sense' — one only when the differences were large, and one even when they were small — while five had to *reflect* or go through a process of comparison. Most subjects used a variety of *secondary criteria*.

The most frequently used were *movements* of the body or the bodily members (by all except three). The most frequent movement was by the finger, followed by the head, foot, body, tongue and throat. One kept time by articulating more or less for the two standards. One nodded the head for the series which had just elapsed in 104-144, and for every second click in 40-208, or moved the tongue upward or moved the two fingers for each beat. One swayed the body with the slow clicks, and one contracted the throat to retain the rate of the first standard. Three kept the first standard in mind by moving the finger, two doing this during the variable series. One moved the finger for the slow standard for the purpose of determining whether two of the variable beats corresponded with one finger movement, but found that the finger beats tended to assume the rate of the variable beats. He also described three lines of different lengths by means of finger movements in the air, but found this of no aid. Three "beat time" more or less for both standards. Closely related was the tendency to count the clicks, but counting proved to be of no aid. For one the rate of counting changed to the rate of the ongoing series.

Almost as many (seven) tried to *halve* or *double* the standards; i. e., to select a rate which was half as fast as the faster standard and twice as fast as the slow. One proceeded to 'multiply the slow by two and divide the fast by two'; one 'grouped the faster into pairs and doubled the slower ones'; one tried to see if there were two beats in the variable for one movement of the tongue for each beat in the first standard, but the change to the second standard made him uncertain. The use of this factor was restricted largely to the 40-208 pair (mentioned five times, but only two times with 72-176 and once with 104-144).

Theoretically we should assume that this expedient was useless or misleading, for the process of doubling or halving does not give the correct mid-rate, as is seen from the following tabulation in which the faster rates in each pair are halved and the slower rates doubled:

Standards	40-208	72-176	104-144
Double the slower. . . . .	80	144	208
Half the faster. . . . .	104	88	72
	92	116	140

In practice, the mid-tempos actually selected were sometimes nearer the true mathematical mean, and sometimes nearer the halved or doubled rate, or the average of the halved and doubled rates. The results for the same subject were not always consistent. In all cases this factor increased the amount of the error. While we may thus say that it exercises an influence on the choice of the mid-tempo, particularly for the 40-208 pair, it does not do so to the same extent in all the part series: in one combination the subject may rely on this aid but not in another. This fact evidently accounts for the large difference between some of the part series found for some of these subjects.

The use of *visual imagery* was reported by three only. One imaged the pendulum weight at the top, middle and bottom of the pendulum for the three series, and also a line on the blackboard for the mid-rate; one had a vague perception of the distance between each beat; and one had a very vague impression of visual extent. On the other hand, five did not use any sort of a visual scheme. It is therefore probable that visual imagery did not exercise any determinate influence on the estimations.

As already seen five believed that they were, and five that they were not, able to *ideate* the mid-rate, to have in mind a *mental standard*—a sort of intuitive mental yardstick—by means of which to determine the mid-rate. The existence of such an 'ideal standard' can scarcely be postulated until this phase of the problem has been subjected to a more searching inquiry. There is material aplenty here for an independent investigation.

One thought that he *knew the rates* of the limiting stimuli in the 40-208 pair, and that he used this knowledge as a criterion, while two felt that their later estimations were influenced by earlier ones—such influence is discernible in some combinations but certainly not in others.

Two employed *objective criteria*, one conceiving of the mid-rate as twice as fast as the second's hand, and one having in mind a clock tick.

One *grouped* the first standard and variable, and then the variable and second standard and recalled the *contrasts*. He based his judgments on the 'contrasts.' One perceived the mid-tempo as a 'smooth rate,' while he was unable to adapt himself to the standards, particularly the very fast. One tried to keep both standards in mind, and to image a mid-rate 'which was neither very fast nor very slow.'

The *breathing* or *pulse rate* did not exercise any influence with any of the five subjects reporting on this point.



In view of the use of a variety of secondary criteria we seem justified in *concluding* that the process of equating appertains to the sphere of *reflective judgment* rather than to the sphere of *immediate sensation*. Probably fewer subjects than indicated by the introspections based their judgments purely on the immediate impressions of the stimuli (sensational factor). The criteria actually used varied more or less with the different subjects and the different standards, and also in their degree of effectiveness. The *movement* factor was perhaps the most effective, while the 'doubling' and 'halving' process led the judgment astray.

7. From other introspections the *following facts* appear. The tendency to *rhythmically group* the individual series of sounds was not very pronounced (reported by four only). The accuracy of the rhythmical combinations was not superior to the general averages, or to the subjects' non-rhythmical combinations, except in two cases.

The subjects were asked whether the series of beats appeared to them as '*durations*' (intervals) or '*frequencies*.' Two found with the fast beats that attention was on the 'number of beats,' or the 'rapidity,' or the 'frequency,' while with the slow beats one found that it was on the 'duration or length of the interval.' Four others seemed to regard the series as durations.

Attention seems to be directed nearly as often to the *sensation* as the *stimulus* sides of the experience. Seven attended more to the sensations in one combination or another (one explaining that he did not 'have the instrument in mind,' and one attending to the beats as they 'affected him') while five attended more to the stimuli in one combination or another (one 'visualized' the metronome).

At what point in the three series did the subjects form their opinion or arrive at their *decisions*? Nine usually or at times decided during the *course of the variable series* of beats, some in the early part and some in the latter part. Six did this only when the variable was not too near the mid-point. The decisions occurred more frequently in the variable series for the 104-144 (four mentions) and 72-176 (three

mentions) pairs than for the 40-208 pair (once). Of course, the decisions could be reached in the variable series only after the variable standard had become so familiar that it could be retained.

Ten arrived at their decisions usually or at times during the *course of the second standard*, more frequently in the early part than in the latter part of the series, and more frequently with the 40-208 pair (six mentions) than with the 72-176 and 104-144 pairs (four mentions each). Two had to wait for the second standard when the variable was near the mid-point; one had to wait because it was difficult to remember the 40 standard, which he usually ideated too fast; and one, after hearing the first combination in a pair, used the second standard only to verify his judgments. Two sometimes made their decisions *after the close of the second standard*—one when the variable was near the mid-point or when he was doubtful, and the other when his judgments would change during the latter part of the second standard.

While it is thus apparent that there is no one favorable moment in which the decisions are reached, the *nearer together the limiting stimuli are* (within our limits) *and the farther the variable is from the mid-point, the earlier is the judgment formed*. Most subjects decide in the variable series or in the early part of the second standard. Extremely few wait until all three series have been given.

## SECOND EXPERIMENT

### *The Estimation of the Mid-Rate between two Tempos by the Method of Tapping* (Method of Expression).

The instructions given were as follows: "I shall have the metronome click off a series of tempos, and then, after an interval of three or four seconds, a second series of tempos, which we shall refer to, respectively, as the first and second standards (or fast and slow, or slow and fast standards). When the metronome has been stopped and I have had time to start the kymograph, I want you, at a given signal, to tap off on this key, with a quick movement of the fore-

## TABLE VII

Standards: 40 and 208 (Beats per Minute)

S.	Time Order	Length of Interval Between Taps																Average, First to Fourth Sets			
		First Set				Second Set				Third Set				Fourth Set							
		Phases		M. V.	Total Length	Phases		M. V.	Total Length	Phases		M. V.	Total Length	Phases		M. V.	Total Length				
		Down	Up			Down	Up			Down	Up			Down	Up						
J. ....	I.	.175	.247	.423	.050	.152	.232	.385	.007	.268	.436	.073	.201	.445	.035	.162	.267	.430	.041		
H. ....	III.	.068	.826	.895	.031	.070	.868	.938	.026	.072	1.060	1.132	.028	.070	.842	.912	.039	.899	.969	.031	
D. ....	V.	.245	.368	.613	.022	.173	.291	.466	.013	.207	.317	.525	.015	.142	.467	.611	.024	.189	.361	.554	.018
Ave. ....		.162	.478	.643	.034	.131	.403	.579	.015	.149	.548	.697	.038	.121	.533	.656	.033	.140	.509	.651	.030
Wa. ....	IV.	.316	.203	.510	.015	.306	.230	.536	.017	.295	.283	.578	.026	.271	.272	.544	.028	.297	.247	.544	.021
S. ....	II.	.036	.978	1.015	.046	.028	1.036	1.065	.036	.028	.962	.991	.018	.031	.992	1.023	.035	.991	.992	1.023	.033
R. ....	IV.	.214	.264	.478	.019	.202	.265	.467	.013	.237	.260	.497	.012	.218	.263	.482	.015	.218	.263	.482	.015
B. ....	II.	.013	.366	.380	.011	.021	.381	.402	.107	.035	.303	.338	.010	.022	.365	.387	.027	.023	.354	.377	.039
Ma. ....	II.	.105	.530	.635	.012	.115	.672	.787	.031	.181	.457	.638	.023	.217	.638	.856	.037	.154	.574	.759	.026
Ave. ....		.137	.468	.603	.020	.134	.516	.651	.041	.155	.453	.608	.018	.170	.425	.595	.030	.144	.486	.631	.027
Ave. <sup>1</sup> ....		.140	.472	.618	.020	.133	.497	.624	.031	.153	.488	.642	.025	.145	.479	.625	.031	.143	.494	.638	.028
Standards: 208 and 40																					
Wa. ....	III.	.425	.266	.691	.014	.371	.230	.603	.015	.390	.221	.610	.029	.313	.235	.551	.016	.375	.238	.613	.018
S. ....	I.	.037	1.031	1.068	.060	.039	1.142	1.181	.046	.040	1.145	1.185	.023	.031	1.030	1.061	.023	.037	1.085	1.122	.038
R. ....	III.	.166	.234	.401	.040	.204	.226	.431	.012	.156	.255	.412	.011	.204	.264	.468	.021	.182	.245	.428	.021
B. ....	I.	.016	.293	.310	.011	.016	.375	.392	.025	.013	.388	.402	.017	.018	.630	.648	.025	.016	.416	.432	.019
Ma. ....	I.	.138	.675	.814	.036	.090	.592	.682	.020	.070	.673	.743	.033	.218	.712	.931	.043	.129	.663	.791	.035
Ave. ....		.156	.499	.656	.032	.144	.513	.657	.023	.134	.536	.670	.022	.157	.574	.731	.025	.148	.529	.677	.026
J. ....	II.	.175	.460	.635	.037	.138	.457	.596	.023	.187	.550	.737	.035	.148	.687	.836	.033	.161	.540	.703	.032
H. ....	IV.	.032	.785	.820	.032	.045	.757	.805	.011	.037	.678	.716	.055	.067	.734	.801	.034	.045	.738	.785	.033
D. ....	VI.	.201	.325	.526	.013	.175	.275	.452	.021	.142	.271	.413	.009	.080	.283	.363	.011	.149	.289	.441	.013
Ave. ....		.136	.523	.660	.027	.119	.496	.617	.018	.122	.499	.622	.033	.098	.568	.666	.026	.118	.522	.643	.026
Ave. <sup>1</sup> ....		.148	.508	.658	.030	.135	.506	.642	.021	.129	.522	.652	.026	.134	.571	.707	.026	.136	.526	.664	.026
Ave. <sup>2</sup> ....		.147	.490	.638	.028	.134	.502	.633	.026	.141	.506	.647	.025	.139	.532	.672	.028	.139	.510	.651	.027



# SECOND EXPERIMENT—ESTIMATION OF THE MID-RATE BETWEEN TWO TEMPOS, BY THE METHOD OF TAPPING

## TABLE VIII

Standards: 72 and 176

S.	Time Order	Length of Intervals Between Taps												Average, First to Fourth Sets				
		First Set			Second Set			Third Set			Fourth Set							
		Phases	Total Length	M. V.	Phases	Total Length	M. V.	Phases	Total Length	M. V.	Phases	Total Length	M. V.	Down	Up			
S	V.	a	.021	.711	.022	.027	.603	.031	.030	.576	.010	.020	.515	.015	.024	.601	.626	.034
H	V.	a	.053	.708	.020	.073	.758	.019	.156	.761	.031	.236	.637	.033	.129	.716	.845	.025
D	I.	a	.065	.251	.010	.091	.245	.082	.190	.295	.013	.152	.275	.011	.123	.267	.391	.029
Ma	III.	a	.112	.555	.027	.096	.403	.019	.105	.537	.033	.075	.707	.028	.097	.545	.642	.026
Ave.			.062	.531	.019	.072	.502	.052	.120	.542	.022	.120	.533	.022	.093	.532	.626	.028
I	IV.	b	.196	.465	.024	.244	.363	.013	.303	.423	.010	.203	.295	.023	.238	.384	.621	.017
Wa	II.	b	.294	.253	.031	.291	.213	.030	.311	.221	.019	.281	.231	.024	.294	.229	.524	.026
R	II.	b	.185	.220	.016	.202	.236	.009	.184	.277	.012	.183	.267	.011	.188	.250	.438	.012
B	VI.	b	.023	.665	.020	.014	.557	.015	.099	.606	.031	.022	.460	.048	.018	.572	.590	.051
Ave.			.174	.400	.075	.187	.343	.039	.203	.382	.018	.172	.313	.026	.184	.359	.543	.026
Ave. <sup>1</sup>			.118	.478	.021	.120	.422	.045	.161	.462	.020	.140	.423	.024	.138	.445	.584	.027

Standards: 176 and 72

I	III.	.242	.548	.791	.026	.185	.510	.695	.021	.186	.716	.902	.028	.194	.443	.637	.035	.202	.530	.733	.027
Wa	I.	.250	.285	.540	.013	.280	.270	.550	.025	.307	.238	.541	.045	.272	.262	.540	.028	.016	.264	.541	.028
R	I.	.045	.375	.421	.013	.060	.358	.418	.021	.165	.281	.446	.015	.168	.262	.431	.016	.016	.317	.426	.016
B	V.	.023	.497	.521	.021	.021	.694	.715	.019	.013	.466	.480	.056	.016	.430	.446	.037	.018	.521	.539	.033
Ave.		.140	.426	.558	.018	.136	.458	.594	.021	.168	.425	.502	.036	.162	.349	.513	.029	.029	.408	.559	.026
S	VI.	.026	.498	.524	.029	.027	.555	.583	.013	.023	.493	.516	.014	.020	.048	.509	.011	.024	.508	.532	.017
H	VI.	.086	.726	.813	.017	.135	.641	.776	.022	.227	.503	.730	.013	.108	.657	.767	.019	.139	.632	.771	.018
D	II.	.146	.237	.383	.012	.141	.336	.477	.022	.188	.228	.417	.024	.145	.230	.375	.007	.155	.258	.414	.016
Ma	IV.	.053	.765	.818	.023	.093	.627	.721	.033	.056	.661	.717	.020	.036	.632	.667	.031	.060	.670	.731	.027
Ave.		.077	.556	.634	.020	.099	.530	.630	.022	.123	.471	.595	.017	.077	.399	.570	.017	.004	.517	.612	.019
Ave. <sup>1</sup>		.109	.491	.596	.019	.117	.498	.610	.022	.145	.448	.593	.020	.132	.374	.546	.023	.121	.462	.585	.022
Ave. <sup>2</sup>		.113	.485	.566	.020	.123	.460	.584	.033	.153	.455	.608	.023	.132	.368	.557	.023	.122	.453	.584	.024

TABLE IX

Standards: 104 and 144

S.	Time Order	Length of Intervals Between Taps												Average, First to Fourth Sets				
		First Set			Second Set			Third Set			Fourth Set							
		Phases		Total Length	M. V.	Phases		Total Length	M. V.	Phases		Total Length	M. V.	Phases		Total Length		
		Down	Up			Down	Up			Down	Up			Down	Up			
Wa.....	V.	.294	.210	.504	.027	.298	.513	.027	.298	.511	.012	.295	.246	.542	.018	.294	.514	
S.....	a	.020	.621	.641	.028	.024	.613	.637	.012	.020	.555	.015	.021	.588	.010	.594	.015	
R.....	V.	.226	.254	.481	.023	.232	.213	.445	.012	.222	.236	.009	.233	.228	.462	.028	.228	.462
B.....	a	.018	.494	.513	.033	.020	.433	.473	.034	.022	.570	.025	.020	.592	.012	.523	.543	
Ave.....		.139	.395	.537	.028	.143	.373	.517	.021	.140	.393	.534	.015	.142	.413	.392	.533	
I.....	b	.261	.337	.598	.020	.203	.321	.532	.020	.233	.264	.497	.019	.286	.312	.245	.553	
H.....	b	.047	.762	.810	.027	.036	.700	.736	.024	.042	.737	.780	.014	.030	.690	.722	.762	
D.....	b	.088	.390	.481	.010	.111	.355	.466	.007	.180	.281	.462	.014	.162	.313	.336	.471	
Ma.....	b	.065	.435	.500	.017	.061	.523	.583	.013	.056	.555	.611	.018	.053	.553	.059	.575	
Ave.....		.115	.481	.597	.018	.103	.479	.570	.016	.128	.459	.587	.016	.132	.467	.119	.500	
Ave. <sup>1</sup> .....		.127	.438	.567	.023	.123	.424	.548	.018	.134	.426	.560	.016	.137	.440	.129	.502	

Standards: 144 and 104

I.....	V.	.313	.437	.751	.026	.212	.366	.578	.041	.158	.365	.524	.035	.225	.388	.613
H.....	I.	.032	.775	.807	.057	.042	.823	.866	.049	.036	.693	.730	.052	.033	.716	.752
D.....	III.	.175	.232	.407	.012	.191	.265	.456	.013	.172	.243	.415	.011	.141	.268	.414
Ma.....	V.	.052	.498	.551	.014	.034	.511	.545	.007	.042	.467	.510	.017	.047	.485	.533
Ave.....		.143	.485	.629	.027	.119	.491	.611	.027	.102	.442	.545	.029	.111	.402	.578
Wa.....	VI.	.298	.222	.521	.023	.294	.248	.541	.016	.264	.256	.520	.015	.274	.259	.533
S.....	IV.	.020	.500	.520	.020	.021	.478	.500	.020	.023	.446	.470	.015	.222	.243	.450
R.....	VI.	.236	.241	.487	.014	.226	.247	.475	.014	.243	.245	.488	.012	.224	.248	.472
B.....	IV.	.023	.725	.748	.039	.020	.542	.562	.034	.025	.456	.476	.040	.020	.554	.574
Ave.....		.144	.422	.569	.024	.140	.370	.519	.022	.139	.349	.488	.020	.135	.371	.507
Ave. <sup>1</sup> .....		.143	.453	.599	.025	.120	.435	.565	.024	.120	.395	.516	.024	.090	.411	.509
Ave. <sup>2</sup> .....		.135	.445	.583	.024	.126	.429	.556	.021	.127	.410	.538	.020	.118	.427	.546
															.425	.555

Explanations follow Table IX.

*Explanation of Tables.*—Unit of measurement: 1 second. S.: subject (males). Time order: temporal succession in which the different combinations were given. I. = first. II. = second, etc. *a* and *b* indicate which of the standards in a given pair, the fast or the slow, was given first; *a* = first; *b* = second. Phases: refers to the *down* and *up* movement of the finger in tapping the key. Total length: the total length of the tapped mid-interval, the distance from the beginning of one tap to the beginning of the next, thus including both the down and the up phases. First set: this gives the average results for the first series of eight or nine taps. Second set: ditto for the second series of eight or nine taps, etc. The one set followed the other in close succession. M.V.: mean variation, computed between the individual taps for each subject in each set. Ave.<sup>1</sup>: the average for the fast-slow or slow-fast combination of a given pair. Ave.<sup>2</sup>: general average for a given pair, average of the two Ave.<sup>1</sup>'s. Pair: the two limiting stimuli which were given together; *e. g.*, 40 and 208. Combination: the order in which the standards (limiting stimuli) were given; *e. g.*, '40 and 208' followed by '208 and 40'; or '208 and 40' followed by '40 and 208.' The standards were given in the order indicated. Thus '40 and 208' indicates that the 40 rate preceded the 208 rate.

finger of the right hand, a tempo which you estimate to lie midway between the rates of the standards—that is, the mid-tempo.” The kymograph drum was allowed to attain its speed before the subjects were asked to tap—an interval of five seconds, more or less, after the second standard had been given.<sup>1</sup>

The electrical tapping reaction key was so constructed that the electrical circuit was closed the moment the key was depressed, and broken again the instant the key returned (by spring action) to its original position. In Tables VII. to IX. the time the key was depressed ('down' phase of the finger movement) and the time it was at rest ('up' movement of the finger), are given separately. The length of the estimated mid-interval is, of course, the sum of the make and the break records on the drum—that is, the entire time from the beginning of one tap to the beginning of the next.

The reactions were recorded by means of a Deprez marker on the smoked paper on the drum of a Ludwig-Baltzar kymograph. Time lines, made by a 100 VD fork, paralleled all the reaction records. The measurements are accordingly in terms of seconds. It is thus necessary to express the mid-

<sup>1</sup> Three regarded this interval as about right (S, H, R), and two sometimes felt that it was rather brief (D, Wa).



rates in the tables as *interval lengths* or *durations* in fractions of a second, instead of as *frequencies* in terms of the number of beats per minute, as was the case in the first experiment.

After the subject had tapped eight or nine times the kymograph was stopped, and the subject was required to listen to the same standards again, given in the same order, after which he was asked to tap off his estimated mid-rate just as before. This procedure was gone through two more times (in almost all cases) so that the subjects listened to the same pairs of standards in the same order, and tapped the estimated mid-tempo, in four successive sets. The successive sets of eight or nine taps are averaged in the tables in the columns headed 'First Set,' 'Second Set,' etc. It is therefore apparent that altogether eight successive sets of measurements were made for each pair of standards: 4 sets in the fast-slow combination (*e. g.*, 208-40), and 4 sets in the slow-fast (40-208)—or 24 sets for all three pairs.

*Results.*—(1) The produced mid-intervals, based on the averages for the four sets<sup>1</sup> (last four columns), are invariably *longer (slower) than the arithmetical mean*. The error, in terms of interval length, amounts to .168 sec. for the 40-208 pair, .101 sec. for the 72-176 pair, and .071 for the 104-144 pair. In all averages the mid-rates are *nearer the geometrical than the arithmetical mean*. The produced mid-interval for the 40-208 pair is exactly the same as the geometrical mean, while it is .054 sec. slower for the 72-176, and .065 sec. for the 104-144 pair.

Three facts here merit attention: first, the fact that the errors in the two experiments are in the *same direction*—the estimated rates being *too slow*; second, the fact that the estimates are *nearer the geometrical than the arithmetical mean*, particularly with the method of production; and third, the fact that the *errors are noticeably larger in the second experiment*—larger by .091 sec. in 40-208, by .084 in 72-176, and by .064 in 104-144. (The estimations in the first experi-

<sup>1</sup> Limitations of space render it necessary to restrict the discussion to the general averages for the four sets (last four columns). More detailed data are available for the reader in the tables.

ment were, in terms of interval length, for the pairs in the order enumerated above, .56, .50, and .49 sec.)

The experimental inaccuracy is paralleled by *introspective uncertainty* in this experiment. Five regarded their productions as guesses, one having no definite idea of the mid-rate, two only being certain that they were somewhere between, and another that his taps were different from the last standard. Only two felt fairly certain. Thus both experimentally and introspectively the method of *tapping* (producing) the mid-rate is less accurate than the method of selecting it from a series of variable clicks. This is probably due to the aid received from the step procedure in the first experiment, for of the five subjects who served in both experiments three regarded the second as easier, and none found the second experiment fatiguing while a considerable number experienced fatigue in the first.

(2) As seen in the above figures, the *absolute inaccuracy increases as the distance between the limiting stimuli increases*, the error for 40-208 being one and two thirds times as large as for 72-176, and two and one third times as large as for 104-144. But relatively to the length of the interval between the limiting stimuli (*relative inaccuracy*) the order is the exact reverse, the errors amounting to 13 per cent., 20 per cent. and 43 per cent. for the pairs in the order given above.

The same difference appears in the mean variations. The absolute M.V's increase as the distance between the stimuli increases (.020, .024 and .027 sec.), while the relative M.V's decrease (.12, .049 and .022).

Two regarded the 40-208 pair as the *easiest* (one because he felt certain with this pair that he was not very near either standard though he did not know whether he produced the correct mid-rate), and two the 72-176 pair. One regarded the 40-208 pair as the *hardest*, and three the 104-144 pair (one because the standards were too close, so that he tended to give one of the standards, one because he was not absolutely sure that he was between, and one because it was hard to retain the fast rate). One expressed no preference, while one regarded all as 'unsatisfactory.'

We therefore conclude that *the absolute inaccuracy of estimating the mid-point increases as the distance between the standards increases*—a uniform result in both experiments. But there is more or less disharmony between the two methods in respect to the *relative inaccuracy* and the *introspections*. Peculiarly in both experiments the pair which gave the least absolute inaccuracy, 104-144, was adjudged the most 'difficult.'

(3) Just as in the first experiment, giving the *slow standard first produced slightly more accurate estimations* than giving the fast standard first, for 40-208 (a difference of .026 sec.)

and 72-176 (a difference of .001), but not for 104-144 (a difference of .013 sec.).

But the M.V's were slightly smaller when the fast standards were given first, except for 104-144; and introspectively the fast was *preferred* first by three, one when the 'faster rate was very fast because it was easier to get a rhythm of this and put it into the slow,' and two because it was easier to base the judgment on the slow. Three had no preference, and one was uncertain.

Five said that they *attended* more to, or were more influenced by, the *last* standard, one 'really making his tapping different from this,' and one feeling certain that his rate of tapping was different from the last standard at least. Two were more influenced by the *slow* standard and one by the *fast*.

Altogether one may conclude from both experiments that *the order in which the slow or fast standards is presented is not very material*. There is a slight difference, but the direction of the central tendency is not very clear. It seems to be largely a matter of personal idiosyncrasy.

(4) The *combination that was given second* for any pair was *more accurate* than that given first.

To illustrate what is implied in this statement: the combination 40-208 was given first to J, H and D (general average .651 sec.), and second (that is, after 208-40) to Wa, S, R, B and Ma (average .631 sec.); while 208-40 was given first to Wa, S, R, B and Ma (average .677 sec.), and second to J, H and D (average .643). The tapped interval lengths for all six combinations were as follows:

	Interval Length	M.V.
First. ....	.617	.026
Second. ....	.589	.022
Difference. ....	.028	.004

It is seen that both the inaccuracies and the M.V's are slightly smaller in the combinations given second in order. There are only two exceptions to the rule among the different combinations (104-144 and 72-176), so far as the average values are concerned, and none so far as the M.V's are concerned (though two are equal). This gain is probably a *practice* effect. When the subject began with the second combination of any pair he already had made about 35 estimations with the first.

(5) The *degree of accuracy* with which different subjects are able to determine the mid-rate *differs considerably*, in this method as in the first. The largest error in the 40-208 pair amounts to .639 sec., which is .156 sec. more than the length of the mid-interval; in the 72-176 pair to .362; and in the 104-144 pair, to .279 sec. On the other hand, six of the sixteen records in the next to the last column (average for all sets) for the 40-208 pair, six for the 72-176 pair, and nine for the 104-144 pair, are correct within approximately .05 sec.



Contrariwise, the *subjects' evaluation* of their own estimations were not very reliable with the second method. Two were fairly certain of their judgments. But the errors in six of the final averages for one of these are large, while the absolute errors in the pair which he considered easiest (40-208) are larger than in the pair which he considered hardest (104-144). The errors for the other subject are all large (S). For the five who were uncertain, fully half of the estimations are fairly accurate for three, while for the other two some of the estimations are approximately accurate.

We may thus conclude that the *individual variations* are greater and the *subjective evaluations* less reliable in the method of expression (tapping) than in the method of impression (selecting mid-tempos from variables). It is well to emphasize the fact, however, that the tapping was very regular for most of the subjects. In the second experiment the M.V.'s could be computed between the individual reproductions in each set for each subject, and for only one do they reach .05 sec. (B, 72-176).

(6) The pressure or *down movement* of the finger in tapping (make phase of records) was considerably *shorter* (excepting for one subject) than the up movement. With the 40-208 it lasted only 27 per cent., with the 72-176 pair 28 per cent., and with the 104-144 pair 30 per cent. as long as the up movement.

(7) The following were the *average values* for the *produced mid-rates* for the *three* standards in each of the *four successive sets* (each set, as stated, consisting of eight or nine determinations):

	First	Second	Third	Fourth
Mid-interval. . . .	.606	.591	.598	.592 sec.

The estimations in the second set are more accurate than those in the first set, those in the third set less accurate than those in the second, and those in the fourth more accurate than those in the third and about the same as those in the second set. In the 104-144 pair all the three later sets are more accurate than the first, but only two each in the 72-176 pair and in the 40-208 pair.

Likewise in respect to the *average regularity* for the three pairs: the M.V. is somewhat less in the third and fourth sets (.027 sec.) than in the first (.024), but greater in the second (.027). In the 104-144 pair, again, the regularity is better in all the three last tests than in the first, but in the 40-208 pair the regularity is better in only two of the later sets, while in 72-176 there is a consistent loss.

It is thus apparent that there was a *slight improvement in the estimations* and a *slight gain in the regularity in the later estimations*—a *slight practice effect*—but neither is regular or consistent except in the 104-144 pair.

(8) The *methods* used to estimate the mid-point were as follows:

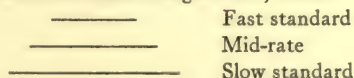
Four believed that they based their estimations on the *immediate impressions* received from the stimuli (one only when the standards were far apart, and one without any reliable or absolute feeling of the mid-rate). On the other hand, three did not think that their judgments were based on the absolute impressions, while two stated that the judgments were of the reflective type—a matter of mental calculation, or of counting.

Practically all the subjects made use of *kinæsthetic factors*—movements of the body, or tongue, or head, or finger. Five used the movement criteria with *both standards*: one moved the head, finger or body (for the slow rates) in time with the standards; one nodded the head or moved the tongue to the first standard and moved the finger to the second standard; three tapped with the finger for both rates, one estimating the mid-tempo from the finger beats. Two felt that the movements or muscular efforts were an aid in that they reinforced the memory, but one felt that they did not help as he tended to beat too fast (his estimations, however, were too fast only with the 40-208 pair). One, on the other hand, moved his finger during the *first standard* only, while one moved his hand to his '*ideal mid-rate*,' but found the value of this to be indifferent. One, who made no conscious movements, counted and found this helpful. One tried to 'measure the duration of the feeling of expectancy' with the 40-rate, and used this as an aid.

Four employed some scheme of *grouping or halving*. One counted the fast clicks in fours and continued to count during the slow standard, to see whether the counts would fall on every second or third click; he sometimes 'grouped the fast into the slow beats and halved the result for the mid-tempo,' and he sometimes tried to 'feel the distance between the third and fourth clicks.' Another tried to determine 'how many fast clicks went into a slow interval and took one half of the result,' but this did not prove satisfactory except for 40-208. One counted in eights, probably only as an involuntary tendency to rhythmize, and one 'grouped the 208 rates in halves' but found this a hindrance.

Two believed that they were, and six that they were not, guided more or less by an '*ideal standard*' for the mid-rate (one got in his 'auditory imagination' an image of the 'rate between'). One 'merely had an impression of what it ought to be.'

Only three were conscious of using any *visual imagery*: one visualized the tapping of a hammer; one visualized the mid-rate as a line dividing off a spherical area; and one had a vague image of a truncated triangle effect, thus:



Two felt that the produced mid-tempo for one pair influenced the judgment for the other pairs. One considered this a habit effect. The alleged influence is perhaps apparent in 'R's' averages, but only slightly, if at all, in 'Ma's.' 'Ma,' however, had a tendency to change his tapping rate, because he felt that he was beating too fast, but his M.V's are larger than the average only in some cases.

Comparison of the two methods (first and second experiments) justifies the following *conclusions* respecting the *process* of estimating the mid-tempo:

First, all subjects rely on some form of *secondary criteria*. Some try a considerable variety of aids, others only a few. By a process of selection and elimination, aids found useless are abandoned and those believed to be valuable are retained. The reactions are of the 'excess discharge' type.

Second, accordingly—although about half the subjects in each experiment report that the estimations were based on the immediate impressions received from the stimuli, and although about half in the first experiment and one third in the second report that they were guided more or less by an ideal standard of the mid-rate—the *judgments are mainly, if not entirely, of the reflective type*.

Third, the *stable criteria* in both methods may be reduced to two, namely kinæsthetic or motor factors, and a process of halving or doubling the standards (grouping). In the first method the movements seem to be made more frequently in time with the first standard, while in the second method the movements are usually made for both standards.

Fourth, *visual imagery* plays a very subordinate rôle in estimating the mid-point between tempos.

Fifth, the *value* of the different criteria employed varies, as shown by the experimental results and the introspections, with different subjects and with the distances between the pairs of standards.

(9) The *difficulty* experienced most frequently was the *inability to retain the standards*. Two found it particularly hard to retain the *first standard* (the 'second tended to drive out the first'); and one to retain the *fast* when the standards were close together.

Three found it difficult to get any impression of the *mid-rate*, and four to avoid the tendency to *tap too fast*. As a matter of fact, five of 'R's' and 'D's' and two of 'B's' six estimations (general averages) were too fast, while all of 'Ma's' and four of 'B's' were too slow. One found it difficult to keep from *basing his judgments* on the last standard only (three judged more from the last standard than the first); and one, to avoid adopting one of the standards.

Both methods thus make it evident that the *chief diffi-*



*culties in estimating mid-tempos are to retain the standards, particularly the first and the fast, and to ideate, conceive, or know where to locate the mid-tempo.*

(10) Only three seemed clearly to feel any *tendency to rhythmize* the clicks. Of these, all the averages of one, but only one each of those of the other two are superior to the average estimations of all subjects. Four reported that they perceived no tendency toward rhythmical grouping. Both experiments thus show that *the tendency to rhythmize a uniform series of clicks in an experiment of this sort is weak, and that, when present, it usually does not increase the accuracy of the estimations.*

(11) Six subjects apprehended the succession of clicks as *frequencies*. (One only the rapid clicks, and one the 40-208 pair.) 'Ma's' tendency to count indicated to him that he apprehended the series as frequencies. On the other hand, three apprehended the rates as *durations* or interval lengths, one only the slow standards, and one the two pairs of standards closest together.

The two methods agree in indicating that there is a tendency to regard the *fast series as frequencies* and the *slow as durations*, but that the subjects often are *uncertain* as to whether they apprehended the series in the one way or the other. It is observable that in the first method more regarded the beats as durations while the reverse was the case in the second method.

Whether the one type of apprehension makes for *greater accuracy* of estimation than the other is difficult to determine, because many subjects will regard one standard of a given pair as durations and the other as frequencies. If we consider only those who apprehended the successions in one way, and sum up the number of individual averages (separately for those who apprehended the successions as durations and for those who regarded them as frequencies) which are superior and inferior to the average estimations for all subjects, it is found that in the first method those who regard the beats as *frequencies* have 11 superior to 12 inferior counts; and those who regarded them as *durations* have 20 superior and 20 inferior counts. The corresponding figures in the second test are, for the frequency group, 15 superior to 11 inferior, and in the duration group 10 superior to no inferior counts. These differences are not material. Some subjects in each group excel in all their averages, and others are always poor, while some subjects in each group are better than the average in some pairs but not in others.

It is thus apparent that so far as concerns the accuracy

of estimation it is *immaterial whether the clicks are apprehended as frequencies or durations*.

(12) Two subjects believed that they *attended* more to the stimuli than the sensations while five did the reverse. One centered the attention mostly on his counting.

Both experiments show that the *subjects are not always certain* as to whether their attention is mainly objective (by which I refer to attention mainly directed to the stimuli) or subjective (directed to sensation), that the same subject may pronounce differently at different times or for different pairs, but that, in the main, *attention is centered more on the sensation side than on the stimulus side*.

Here again different subjects center the attention differently at different times so that it is difficult to determine whether the subjective or objective type is the *more accurate*. If we disregard the four in the first experiment who are now objective and now subjective, and sum up the number of superior and inferior averages in each group, as was done above, it is found that in the first experiment the *subjective type* has 13 superior to 21 inferior counts, and the *objective type* 6 superior to 4 inferior. In the second experiment the subjective type has 16 superior to 14 inferior marks, and the objective type 10 superior to 2 inferior. The results are thus discrepant in the two experiments. Some indeed in each type are uniformly accurate and others are uniformly inaccurate.

We conclude that the *accuracy of estimating mid-tempos does not depend on whether the subject attends chiefly to the sensation or the stimulus side of the experience*.

(13) Three arrived at their '*decisions*' *after the close of the last standard*, while two decided *toward the close of the second standard*. Three scarcely regarded the matter in the light of a *decision*: one simply struck off a rate and then judged of its satisfactoriness; one found it impossible to form a decision and simply waited for the signal to tap; one felt that to have formed a decision he would have had to take time to make a comparison.

The instrumental procedure in the two experiments was so different that a comparison on this point would be meaningless. But it appears that *in the first method the subject goes through more of a process of 'forming an opinion or reaching a decision,' than in the second method*.

## LITERARY SELF-PROJECTION

BY JUNE E. DOWNEY

### I

Consideration of the imaginal forms that the self may assume would appear essential to a complete understanding of the psychology of the self. Yet this specific form of imagery has been unduly slighted.

A popular article by the present writer has described<sup>1</sup> the conceptual images of the self as reported by a number of observers. The most common representation appeared to be visual, and of such visualization the most frequent forms were reproductions of the reflection in the mirror or of a favorite photograph, the latter of which might be absurdly out of date, unduly flattering, or the reverse. The shadow also was found to be a frequent symbol of the self. In the visual images of the self peculiar abbreviations of the person might occur, so that the visualized self appeared decapitated or in schematic form, reduced perhaps to a pair of feet or one shoulder, or described as a dark square topped by a pompadour. Curious substitutions occurred, as in the case of the sister whose visual representation of herself took the appearance of her brother. The concept of the self was often described as an attitude; the attitude, for instance, of meditation—a shadowy chin felt to be resting against clasped hands.

Now the report of such symbols must have value as a revelation of temperament. This is evident in many instances as in that of the diffident woman whose self-image is always clad in an unbecoming coat, or in that of the young girl who always appears in her mental rehearsals gowned in her favorite party dress. A young man, with an ambition to be a physician, includes in his visual representation of himself a doctor's case. A college girl whose concept of

<sup>1</sup> 'The Image of the Self.' *The Educational Bi-Monthly*, Vol. IV., No. 1 (1909).



self is described as the feeling of a semi-masculine stride finds that this idea of herself dominates her in her selection of clothes even to the detriment of her judgment as to what is becoming to her objective self.

Some interesting facts relative to the representation of the self in a particular situation have been reported by Professor Martin<sup>2</sup> in her investigation of the images serving as cues to voluntary movement. Thus the transfer of the visualized movement to another person or to an animal was found to occur not infrequently. The writer's own experience as subject in a similar test seems instructive. A visual report from the body is found to be present always and the visual cue for a movement and the movement itself become inhibited if the part of the body that is to be moved is in a contradictory position. Frequently a double visualization appears. Thus, the command, "Raise both arms," brings a clear visual report of the two arms stationary with another shadowy pair in motion. The bearing of such facts upon the individual volitional type must be important.

## II

The present report concerns itself with the personal reference involved in the appreciation of literary material and, in general, bears upon the question of the relation of the self to æsthetic experience, a question that, theoretically, has been productive of much discussion.

The situation utilized in gathering the reports was an extensive report upon the imagery aroused by the silent reading of fragments of poetry or by hearing such fragments read aloud. Two groups of subjects were used. There were eight subjects in the first group, all of whom had some training in experimental introspection. The second group contained six subjects of more extensive psychological training than those of the first group.

The members of the first group (*A-H*) reported on one hundred and ten fragments of poetry read silently, and

<sup>2</sup> Martin, L. J., 'Zur Lehre von den Bewegungsvorstellungen.' *Ztschft. für Psychol.*, Bd. 56 (1910).

on forty more fragments read aloud to them. Among the instructions given them was one taken from Professor Martin's investigation "Über ästhetische Synästhesie."<sup>1</sup> "Do you experience a posture or movement of an object described as if it were your own posture or movement? If so, in what part or parts of the body is the posture or movement felt?"

In addition to such report the subject was asked, several weeks later, to arrange the hundred and ten fragments in three groups relatively to the vividness with which the subject projected himself into each fragment, with a fourth group for the fragments in which no self-projection occurred. An introspective report on the kind of self-projection was also asked for, self-projection being defined as any form of explicit reference to self, relative to the imaginal content. From this grouping the experimenter was able to determine the frequency and vividness with which self-projection was experienced.

The second group (*I-N*) reported the imagery aroused by the silent reading of one hundred fragments, the personal reference being inferred indirectly from detailed questions relative to the imagery aroused. In addition, thirty-one fragments were read silently with instruction that self-projection be reported whenever it occurred. The following forms of personal reference were noted as possible: Definite orientation relative to the imaged scene; felt posture; felt movement; organic identification with character or object described; cutaneous reference; visual and auditory self-reference.

It is evident that the situation made it possible to note cases of æsthetic *Einfühlung* or empathy with special reference to an imaginal content aroused by poetic material. No questions, however, referred specifically to such empathy and only one subject (*M*) of the fourteen gave answers whose phrasing showed familiarity with the doctrines of *Einfühlung*.

The self-projection on which a report was sought was, however, more inclusive than empathic experience. Visual

<sup>1</sup> *Zeitschrift für Psychologie*, Bd. 53 (1909).

self-projection in which, for instance, the subject sees himself as part of a visualized scene or as spectator of another's actions is an impressive form of self-projection but very different from an empathic projection. Again, although such self-projection is more inclusive than empathic projection, it is less inclusive than self-consciousness.

A general survey of the reports from the fourteen subjects shows, first of all, a noteworthy difference in the number of times explicit self-reference is reported. *C*'s number of self-projections is only 19.3 per hundred; *E*'s is 62 per hundred.

It is also interesting to note that such self-projection occurs with least frequency, in the reports both of the first and of the second group, in just that subject (*C* of the first, *K* of the second group) who gives the greatest number of memory images. The memory images of *C* bear chiefly the place-index, those of *K*, the time-index; for neither is the personal reference acute. The memory-consciousness appears often to be as objective in its reference as is a perceptual consciousness.

The first form of self-projection to be considered in detail is that of visual self-projection, or the seeing oneself in the midst of a visualized scene as actor or spectator. Rusk<sup>1</sup> in his tests on mental association in children found visual self-projection in the boy's imagery with what seemed to him surprising frequency. Several adults who were subjects in the present test reported visual self-projection as frequently as did Rusk's boy-subjects. In the tests where questions were asked specifically as to self-projection, *J* projected himself visually twenty-six times out of a possible thirty-one.<sup>2</sup>

Some details relative to this form of self-projection are of interest. Usually the self is seen as a more or less vague figure of the proper sex with little that is specific in the way of facial or other detail. For *B*, the self was merely a vapory figure seen in a given posture. *F*'s visualization of the self

<sup>1</sup> Rusk, R. R., "Experiments on Mental Association in Children." *The British Journal of Psychology*, Vol. III., Part 4 (1910).

<sup>2</sup> A curious example of what appears to have been visual self-projection of hallucinatory vividness is to be found in the biographic accounts of Shelley's apparitions of himself.



was slightly more specific and always clad in a reproduction of the garments she was wearing at the time of report. *E*'s visual self-projection was very specific. This self was definitely placed and seen from a definite position. Sometimes, this self would appear close at hand and life-sized. On such occasions the details would be vivid and a complete description could be given of the appearance of the self, such as style of hat worn, color of dress and the like. At other times, the self appeared far off, from half a block to half a mile. It would then seem indistinct and reduced in size. The following reports show the nature of *E*'s self-projection.

Visual, dark picture; by the sea; wind blowing. Optical movement of sea. Visual and felt self lying by the sea. Felt the wind on the face. Self seen from afar off; dim figure.

Again,

Visualized and felt self on the shore; optical movement of waves. Saw and felt the sand falling through fingers; sad relaxed feeling. Saw self distinctly from behind; self wore white dress and a big floppy straw hat.

*H* is subject to the distance-illusion. At times her surroundings retreat to a considerable distance, becoming smaller as they retreat. This experience of *H* is of interest in this connection because it is evident from her introspective reports that visual self-projection is of frequent occurrence and with this peculiarity, the visual image of self is often very tiny although no distance effect occurs. In general, *H*'s visual images are small and placed about two feet from her. A report of hers follows.

Visual image of self sitting. Dark room. Saw lap, book, and side of chair. Seen to right. Figures reduced in size. The visualized self very small, about eight inches tall, but localized in my actual position, that is, at my side, on chair on which I am sitting.

*H* has a circular form for the months of the year. The circle has a radius of about four feet with *H* localized at the center. If *H* hears a month of the year mentioned, she immediately localizes herself and the visualized scene at the proper place on the circle. Things appearing in the circle are diminutive because so many of them must be crowded into small compass. One of the poetical fragments made mention of the month of June. The report on it reads:

The month of June is to left, in corner of room; its position in the month-form. Self is first projected kinæsthetically to the corner. Then the visual image of self appears, very small, about two inches high, facing east.

*J*, as mentioned above, gave a surprising number of visual self-projections. *J*'s orientation with reference to his visualized self is very definite; so too is his orientation as observer of this visualized self. The distance of the visualized self from the observing self may vary from a few feet to one or several hundred yards, in which case the figure appears properly reduced in size. Such a distinct orientation both of the observing self and of the visualized self that is observed suggests the possibility of a double visual self-projection and such *J* reports as a not infrequent experience. Thus, in anticipating a trip, *J* would see himself going to meet himself. Self number one would be kinæsthetic with a visual glimpse of the feet and self number two a schematic figure approaching self number one from the opposite direction.

*J*'s reports bring up the question of the relation of the visual self to the kinæsthetic or felt self and to other forms of self-reference as, for instance, cutaneous imagery. *J* may feel the posture, movement and tactile sensations or images instead of projecting them into the visualized self. At times, there is an oscillation of visual and kinæsthetic self which fail to fuse as in the following report.

See self from behind standing on beach, facing west. Has just thrown shell into water. Visual and kinæsthetic consciousness of right arm flexed; tension in back felt. No fusion of visual and kinæsthetic consciousness. Sometimes an actual oscillation. Self seen about one hundred yards off. Size reduced one fourth.

Again, *J* may get a double cutaneous report from his position as the observer and as observed.

See self lying on the ground on back. Feel the ground against back. As observer, standing. Get cutaneous sensations from both bottom of feet and back against ground. Cutaneous imagery for both visualized self and observing self.

Another report reads:

Kinæsthetic image of walking behind visualized self which appears as a small barefoot boy walking west. Cutaneous images from bare feet.

The double reference appears again in the following:

I am standing by a chair in which my visualized self is sitting. As observer I feel the movement of tying the handkerchief. As observed I feel its pressure on my forehead.

Visual self-projection was frequently reported by *I* also. Often, however, only a part of the body appeared or there were peculiar complications with kinæsthetic imagery.

To illustrate the first statement:

Image of two hands with pencil, pointing out the color of shell.

Again,

Hint of visual self, headless, with legs but no feet, trunk mainly. Shiver felt but no shiver to visual self.

To illustrate the second statement:

Feel asleep in hammock. Feel self and see hammock but can't see self in hammock.

This failure to visualize self, *I* reports as a curious experience.

At times, *I* reports a fusion of kinæsthetic and visual self; at times, a separation.

The following report illustrates fusion.

Feel of movement projected into the visual image of the self. Standing on left leg; right arm drawn back. Jerk of throwing shell felt.

The next report shows a different condition.

Visual of self sprawled out. Feel of posture belongs to actual body.

Other subjects also reported interesting relations between the kinæsthetic and the visualized self. The following reports were given by *F*.

Feeling of self sitting and dreaming. No other imagery. Visual self to the left of actual self and dressed as I am at present. Posture projected into this visualized self.

Again,

Visual image of self walking through forest. Visual scenery of last summer. Self that is seen is not the self felt walking, the second self is felt in space and back of the other self about one block distant.

*C* reported that the kinæsthetic self was felt to be in the margin of the picture; the visualized self at the focus. For *C*, visual and kinæsthetic self never coalesced; a fluctuation from one to the other would occur. The visualized self was not seen in any great detail and at times led a precarious existence as shown by the following example.

Saw self standing on the shore; mountain cataract; then scene shifted, self still there but changed by close of fragment to another person. At first, a distinct picture of self; at close, just the form of a man.

Self-reference in the form of definite orientation relative



to an imagined scene occurred with great frequency. Sometimes, such orientation is merely a vague feeling of position relative to the objects described; sometimes, further kinæsthetic elements are added. One walks the length, for instance, of a banquet hall and feels beneath the feet the 'soft wool-woofed carpets.'

*B* reports:

Entered room, orientation confusing. Was whirled from center to left of room. Finally, took position in the left corner of a square room. Censers were on either side of room and there was auditory crackling. At last, saw mirrors appearing one at a time in wall, each reflecting a censer. This image caused a dizzying shift to a round room because the mirrors were better suited to a round room. The rapid shift in orientation peculiar. Beautiful room but confusing kinæsthesia.

Tactual self-reference was frequently experienced by other observers than *J*. For several subjects, the mention of wind or rain always induced a cutaneous consciousness of self. Such tactual self-consciousness was particularly noticeable in the case of *A* and *L*. *A* frequently reported that the wind was felt blowing through the hair or brushing the cheek. *A*'s self-projection in the scene described often took the form of tactile images of grasping something with the right hand. This tactile reference accompanies visual imagery of much richness.

I saw a field of grass and flowers with cloud shadows racing over them; then the sea under the clouds. I could see rain and dew lying silver on the grass and leaves, but there was no auditory imagery whatever. There was one tactual image of plunging my hand into the dewy grass.

Tactual empathy was reported by *A* and *L*. Thus *A* writes of the imagery induced by reading the description of a well:

A picture of the well, with big stones around it, and the sunshine dancing in the water and lighting up the walls as if someone had dropped something into it. Tactual images of plunging my hand into the water, and of drinking it.

A description of a fountain always induced a feeling of coldness for *L*, as did the description of snow-clad mountains.

Akin to the tactual self-reference was the acute bodily consciousness brought on by organic suggestion. A heightened consciousness of cardiac, respiratory, and other organic sensations was frequently reported by *L* and *D*. An emotional identification of the self with the emotion portrayed was also frequently experienced by these subjects.

Auditory self-reference was rare. *K* once hears herself laughing, and *J* hears himself whistling once and once hears himself grunting. Probably the best example of auditory self-projection is to be found in those cases where the reader hears each fragment read aloud in his own voice. Thus, *D* hears her own voice in varying cadence. There are emotional fragments in which consciousness is preoccupied wholly with inner elocution. The words read are expressive of her own emotion; she hears and feels herself saying them with dramatic nuances.

The social implication in these reports of self-projection is often evident as shown by the following examples from the reports of *E* and *F*.

Experience myself as dressed in beautiful white things and walking in a shady park. Meet several people who look staring at me but do not speak.

Visual image of a rainbow across the sea. People around self. Visualized self on shore, standing and looking out. People known to be talking. Visualized self feels as though among *many people*.

A report of particular interest in that the fragment, which was descriptive of a rainbow, gave no social suggestion.

### III

How the kinæsthetic self, the feeling of movement and posture, occurred at times in connection with the visual self has already been illustrated. Sometimes the posture or movement was projected into the visualized self; sometimes it was felt as an attitude or movement of the actual self. At times there was an oscillation between the visual and kinæsthetic self, with no fusion of the two.

Self was, however, often projected in terms of felt or imagined kinæsthesia without visual accompaniment or with the visualization of a figure other than the self. Sometimes such attitude or movement appeared curiously objectified and at times was fused with a figure felt not to be the self.

In the following report from *D* we have an excellent example of inner or sympathetic imitation. Possibly under other than experimental conditions the organic complex would have been completely objectified and fused with the visualized posture, for it is obvious that introspective analysis may

prevent complete æsthetic absorption in the æsthetic object. The fragment concerned is one from Keats, as follows:

Upon the sodden ground  
His old right hand lay nerveless, listless, dead,  
Unscathed; and his realmless eyes were closed;  
While his bowed head seem'd list'ning to the Earth,  
His ancient mother, for some comfort yet.

*D* writes:

Perfectly clear-cut visual image of the old man in the posture described. Tactual and kinæsthetic feeling of the sodden ground. Feeling of weight and relaxation in right hand. Kinæsthetic feeling of bowed head and of closed eyes. Auditory attention, with strain in ear.

*M* in reporting on the same fragment suggests the possible loss of self in the object.

Put self into the old man and slight tendency to get outside and see old man.

In contrast to these reports comes *J*.

As observer I am northeast of visualized self and of old man. Visual self about one hundred feet off, looking at old man who is twenty feet farther off. No imitation of old man's posture.

The extent to which such kinæsthetic or organic imitation was felt and given either a self-reference or projected into and fused with a visual or other object of consciousness varied greatly from subject to subject. Such observations are of interest on account of their bearing upon the doctrine of æsthetic *Einfühlung*, particularly with reference to literary material where the object itself is imaginal, an expression in part of the reagent's own type processes, a translation in part of the poet's words.

*M*, who reports no case of visual self-projection and who, in general, has very vague schematic visual images, gave the highest number of empathic reports, particularly in the form of kinæsthetic empathy. He identified himself, in kinæsthetic terms, with waving flowers, palpitating trees, flying insects and the like. Thus after reading a fragment descriptive of a seashell he reports the following delicate reaction.

Emotional tone of iridescence. *Einfühlung*. I become blue and change to silver.

The individual differences may, however, be best appreciated by a comparison of the reports upon a couple of fragments. The first fragment, from Poe, follows:



And I rest so composedly,  
Now in my bed,  
That any beholder  
Might fancy me dead.

The reports may be grouped as follows: (1) No self-projection; reaction impersonal. (2) Visual and kinæsthetic self-reference, with or without fusion of these two forms of self-reference. (3) Kinæsthetic self-reference, with more or less kinæsthetic self-projection.

As examples of the first group, we have the reports of *E* and *G*.

Visual image of a man asleep in bed; face very white,  
and

Visual image of very old lady in bed.

A transitional form between the first and second group is found in *H*'s report:

Visual image of someone in white on a couch. Did not see face. Don't know whether it is self or not. Probably not.

The following reports illustrate the second group.

From subject *I*:

Visual image of self in bed, sprawled out; feel of posture belongs to actual body.

From *F*:

Visual image of self in bed; feel posture of rest.

*J* reports:

Standing near bed; visual self seen on it. Posture felt and contact from bed.

*N* reports:

Self seen in posture; visual, posture, and organic consciousness.

*B*'s reports show the conflict between fusion and failure to fuse of the kinæsthetic and visual elements.

Organic uneasiness. Self seen lying on couch with tapestry cover. At word 'bed' self jumped under cover, kinæsthetic consciousness. Bed is to left. Visual as well as felt self. Felt self is at the left and yet this self is also experienced in the position of actual self. . . . Peculiar organic feeling.

The following reports illustrate the third group:

Subject *L* reports:

Feel the lying posture; cutaneous feel of bedclothes. No visual imagery.

From *M*:

Lying in bed. Feel another person looking at me.

*K* reports:

Posture and orientation consciousness. *Felt* self in middle of room, up in coffin. Self definitely projected.

The second fragment is also from Poe and reads:

Glides spectre-like, unto his marble home,  
Lit by the wan light of the hornéd moon,  
The swift and silent lizard of the stones.

Four subjects reacted with kinæsthetic imagery, that induced to a varying degree identification with the gliding lizard.

Thus *B*.

A crawly feeling. I saw a poisonous-looking lizard writhing his way into a ruined marble hall which had grown slimy and moss-grown from age.

And *G*:

I see a lizard running among the stones on a clear moonlight night. . . . I seem to feel the movements of the lizard . . . as if I were running around on my hands and feet watching for enemies.

Of the other subjects, six projected themselves into the scene. Three of these projections were visual and show no empathic qualities.

For instance, *I* reports:

Visual of self standing, sketchy.

And *J*:

See self lying on ground. . . . Lizard thirty feet away. . . .

*A* and *L* get a tactual self-reference. Thus *A* reports:

A ruined wall among other ruins on a hill, the faint moonlight, the lizard. A feeling that it is a very warm still evening; the image of touching the lizard.

Of the three subjects who give no self-projection, *D*'s report is characteristic.

Visual and optical-movement image of dark lizard gliding toward broken columns, between whose shadows is seen the crescent moon.

## IV

Summarizing, we may say that self-projection may occur in a form that is not empathic. A visual self-projection may be of this nature. The visualized self may be only a spectator of the scene. Such visual self-projection may become empathic when fused with it are projected kinæsthetic, tactual or organic images. That, frequently, such fusion fails is shown by those instances in which the subject feels in person

the kinæsthetic experience and does not project it into the visualized figure of the self. The objectified kinæsthetic or organic factors may, on the other hand, fuse with visual material other than that of the visual self. The visual objectification may take form as a person not the self or assume the form of an animal or some object of the inorganic world. Sometimes there occurs kinæsthetic consciousness without visual accompaniment, and such kinæsthetic consciousness may or may not be objectified.

Relative to this summary, the following questions arise: (1) What is the æsthetic value of self-projection that is not empathic? (2) Which reaction has the greater æsthetic value, the objectification of the kinæsthetic or organic resonance in a visualization of one's self or of another? (3) What value for the æsthetic reaction have localized non-projected kinæsthetic and organic sensations and images? (4) Does *Einfühlung* occur in cases where, apparently, there is no organic resonance whatever? Can, that is to say, a visual objectification of a movement carry the feeling-in tone, as well as, and sometimes better than, a kinæsthetic or bodily resonance?



# A TIME EXPERIMENT IN PSYCHOPHYSICS

BY DARWIN OLIVER LYON AND HENRY LANE ENO

## I. THE PROBLEM STATED

The work here presented grew out of a series of experiments begun in 1911 with a view of ascertaining the cause of the seeming discrepancies that various investigators found in the rate of the nervous impulse, where the results obtained varied all the way from 11 meters per second to 194 meters per second. Realizing that there was no accurate method of differentiating the time taken up in transmission from the time taken up in the cerebral reflex, we decided to use only the sensory nerves and therefore confined ourselves to these alone. While working on the musculo-cutaneous nerve we chanced upon a rather peculiar phenomenon that it is the object of this paper to discuss. Briefly stated this was, that in order to make two sensations fuse in consciousness, the more distal of the two stimuli did not have to be given at nearly so short an interval before the other stimulus, as we would have inferred if the speed of the nervous impulse generally accepted, was even approximately correct. Therefore, in beginning the series of experiments, summarized in this article, the authors were, not only, entirely unaware of the probable outcome, but they had been led, from a study of previous time-experiments, to anticipate a quite different issue from that actually obtained.

Whether the method and results here presented tend to throw some possible light upon the obscure question of the time relation between psychic processes and cortical substrate, must be left to the judgment of the reader.

## II. BRIEF SUMMARY OF PREVIOUS INVESTIGATIONS ON THE VELOCITY OF NERVE CONDUCTION

It is a well-known fact that all actions of the nervous system require a certain time for their accomplishment, and

it is a matter of every day experience that the action of the senses and of the will, are not instantaneous, although they are exceedingly rapid. Though it is commonly recognized that there is a real interval between the mental decision to perform a movement and the actual execution of this movement, it is not so commonly recognized that a certain period also intervenes between a peripheral stimulus and its perception. In the case of a voluntary movement, the time consumed from the volition to the action is occupied by: (1) The act of volition,—taking place in the cortex; (2) The transmission of the motor impulse along the spinal cord and nerves to the muscle end-plates; (3) The action of the end-plates on the muscle with the concomitant, or subsequent action.

With sensations however, although there are also three processes they are not quite the same. They are: (1) Reception of the impression by the tactile corpuscles; (2) Transmission of the sense impulse to the brain; (3) The 'perception' in the brain of the 'sensation.' This may or may not be followed by action.

The first investigator to measure the rate of nerve transmission was Helmholtz.<sup>1</sup> He, like most of the earlier investigators, experimented with the frog, using the classical 'muscle-nerve' preparation. Helmholtz found that the period required for the excitement of the muscular fibers was 1/100 of a second. Later on Helmholtz endeavored to determine the rate of conduction in the nerve and found it to be about 28 meters per second. About the same time Marey<sup>2</sup> performed similar experiments in which he obtained a rate of only 11 meters per second. Lamonsky in 1880 arrived at a rate of 31 meters per second, and about the same time Bernstein got a rate of 25 to 33 meters per second.

All the above, however, refer only to the frog, and it is now well known that the speed of conduction differs in different animals. As early as 1878 Chauveau<sup>3</sup> performed

<sup>1</sup> Helmholtz, *Comptes Rendus de l'Academie des Sciences*, Paris, 1851, tome XXXIII., p. 262.

<sup>2</sup> Marey, 'Du Mouvement dans les Fonctions de la Vie,' 1868.

<sup>3</sup> Chauveau, 'Vitesse de Propagation des Excitations dans les moteurs Nerfs des Muscles de la Vie Animale, chez les Animaux mammiferes,' 1878.

experiments on both the frog and the horse to ascertain their difference in speed of transmission. For the frog he obtained a rate of 21 meters per second; for the horse 65 meters per second.

Burckhardt<sup>1</sup> performed experiments in 1873 on man, using an apparatus in which the beginning and the end of the nervous transmission was registered on a smoked chart. He obtained a velocity of 27 meters per second in the motor nerves of the upper and lower limbs. In the sensory nerves however, he obtained a speed of 47 meters per second. He concluded from the results of a protracted series of experiments that the transmission of voluntary motor impulses in the spinal cord was considerably slower than in the nerves, and believed it to be not more than 11 or 12 meters per second. In 1867 Helmholtz and Baxt obtained an average speed in man of 34 meters per second. Place arrived at a speed of 62 meters per second for the forearm whereas for the arm proper he found it to be about 20 meters per second. Later, Helmholtz and Baxt repeated their experiments and arrived at a rate of 65 meters per second. Contrary to the results obtained by Place, they found the speed of conduction in the median nerve to be slower below the elbow than above. Von Wittich obtained a rate of 30 meters per second.

By the reaction time method, Helmholtz obtained for the *sensory nerves* a speed of 60 meters per second. Hirsch, however, arrived at a speed of only 34 meters per second; while Richet<sup>2</sup> obtained a speed of 50 meters per second.

In 1893 Cattell and Dolley<sup>3</sup> finished an elaborate series of experiments on reaction time in connection with an investigation of the velocity of the nervous impulse. They conclude that their experiments indicate that the velocity must be considerably greater than the commonly accepted rate of 30 meters per second. They show that the cerebral reflex is almost certainly slower when the leg is stimulated than when the arm is stimulated, but that the difference in time

<sup>1</sup> 'Die Physiologische Diagnostik der Nervenkrankheiten,' Leipzig, 1875, p. 32.

<sup>2</sup> Richet, 'Physiologie des Muscles et des Nerfs,' 1882.

<sup>3</sup> Cattell and Dolley, 'On Reaction-Times and the Velocity of the Nervous Impulse,' *National Academy of Sciences*, Vol. VII.



of the entire reaction is much too small to allow for a rate of 30 meters per second in the nerve.

Space will not permit mention of all those who have worked along these lines, but the results obtained by certain investigators not mentioned above will be touched on later.

Our experiment, however, is not particularly concerned with the rate of the nervous impulse, and aside from the fact that our results differ from those generally obtained, our only reason for quoting the above authorities is to show how widely they differ among themselves.

Bloch<sup>1</sup> in 1875 concluded a series of experiments which, though not the same as ours, is along much the same lines. He devised an apparatus by which he attempted to obtain the speed of conduction in the sensory nerves, independent from that of the motor nerves. He discovered that when the finger was stimulated it brought forth a quicker response than when the cheek or the lip was stimulated. He found, when stimulating points at different distances from the brain, that the further away from the brain the point stimulated, the longer the appearance of the sensation in consciousness. He attributed this to the difference in speed of the nervous conduction from different parts of the body to the brain, and explained the quick conduction from hand to brain by saying that the impulse from the hand was capable of a more rapid transformation into a motor response than was one from the cheek for example. He also attributed this difference to a slight variation in the sensibility of the various parts of the body. By timing the application of his stimuli in such a way as to get the two stimuli to fuse in consciousness and thus be perceived at the same time, he found that the more distal of the two stimuli had to be given considerably earlier. This time interval he believed was due, and therefore proportional, to the difference in the lengths of the sensory nerves traversed. From his experiments he concluded that the rate of conduction in the sensory nerves was 192 meters per second, and 194 meters per second in the spinal cord. "His method,"

<sup>1</sup> Bloch, 'Experiences sur la Vitesse du Courant nerveux sensitif chez L'Homme,' *Archiv de Physiol. norm. et path.*, 1875.

says Poffenberger, "was based on the assumption that the fusion of two sensations in consciousness depends only on the time of their arrival in the center and not at all on their points of origin. His figures however, suggest that the time of appearance of a sensation in consciousness may vary according to the part of the body stimulated."

### III. DESCRIPTION OF THE APPARATUS USED

The apparatus can best be understood by studying the accompanying photographs and diagrams. The essential part consists of two disks of hard rubber, half an inch thick and 14 inches in diameter, revolved by clock work. The motive power is a weight of eighty pounds.

The outer disk, Fig. 2, is marked on its circumference with three hundred and sixty degrees. Each disk has a platinum contact,  $1/48$  of an inch wide, set in the edge. When revolving, these contacts pass two other contacts connected with a small storage battery of 8 volts, which, in turn, connects with two electrodes in such a way as to give separate shocks through each electrode as the separate disks pass the contact points (see Fig. 1 and 3).

The disks revolve upon the same shaft, and consequently at the same rate of speed; but the outer disk is adjustable, so that the contacts may be either in the same line, or set apart at any required number of degrees as graded upon the disks. In this way, by counting the number of revolutions a second, and noting the number of degrees that separate the contacts, the shocks can be given at any length of time apart necessary for the experiment. Thus, if the disks revolve once a second, and the contacts are adjusted so that the interval that separates them corresponds to  $1^\circ$  ( $1/360$  of the circumference) the shocks will be given  $1/360$  seconds apart. If the contacts be placed  $60/360$  apart,  $1/6$  second, etc.

The speed is regulated by a brake on the shaft, and also by a second, more delicate, brake on the circumference of the inner disk.

The battery (Fig. 4) is furnished with a disk and pointer to regulate the voltage. The current is passed through sepa-



FIG. 1.

rate induction coils by which the strength of shock through each electrode can be adjusted independently, as shown in Fig. 4.



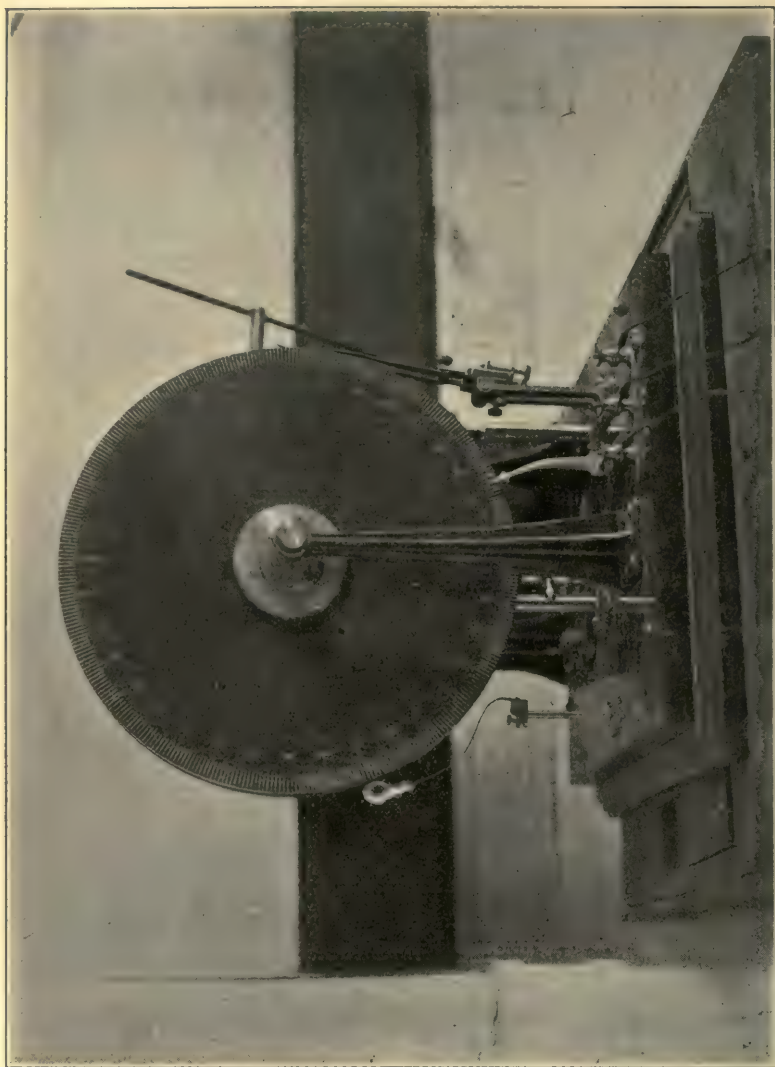
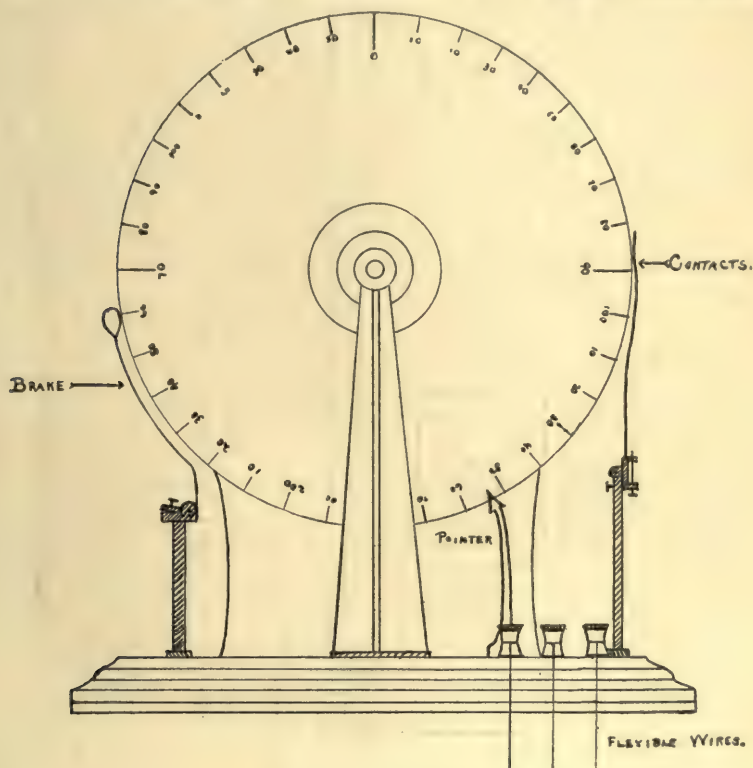


FIG. 2.

#### IV. METHOD OF CONDUCTING THE EXPERIMENT

. During the experiment, one electrode was fastened to the wrist and the other below the elbow, as shown in Fig. 1. The lower of the two stimuli (st.<sup>1</sup>) was given at the wrist-joint in front of the radial artery just above the point where the nerve sends off filaments to accompany the artery as it

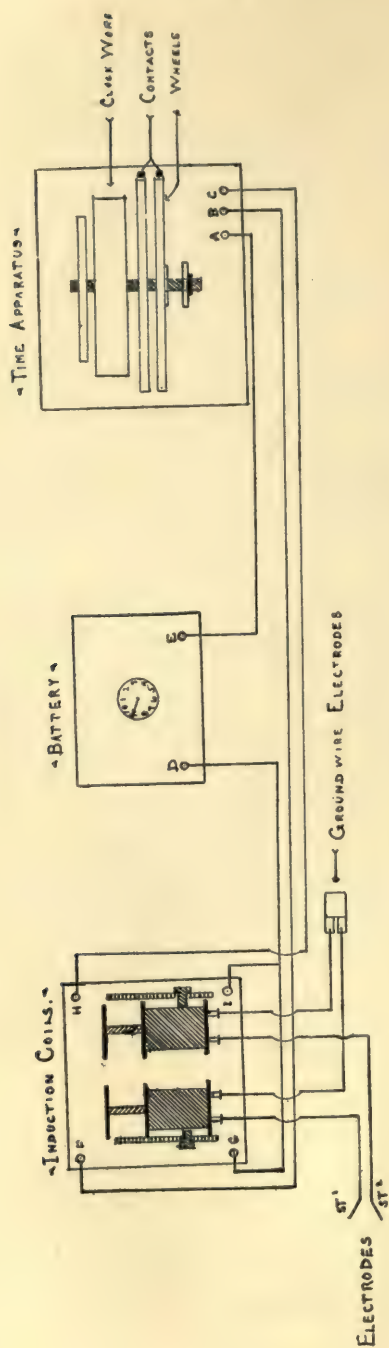
sinks into the wrist. The electrode giving stimulus *st.*<sup>2</sup> was applied to the elbow-joint at the point where the musculo-cutaneous nerve emerges from the deep fascia at the outer border of the tendon of the biceps and becomes cutaneous. The position is well shown in Fig. 5. In performing the experiments the subject was seated with bared arm and in



~ DIAGRAM OF APPARATUS. ~

FIG. 3.

such a position that he could not see the disks. The two stimuli were then given together, when the subject felt the upper shock (*st.*<sup>2</sup>) distinctly before the lower (*st.*<sup>1</sup>). The disks were then gradually adjusted until the shocks were felt to occur simultaneously. In every case the time interval was noted by reading the disks.



-DIAGRAMATIC SKETCH SHOWING METHOD OF WIRING-

FIG. 4.



It was found best, at this point, to slowly move the disks still further apart until the shock from  $st.^1$  was felt first, and then back again, until the two shocks were felt together, thus working from both directions, so to speak, and comparing the reading of the time intervals.

Each subject was also requested to fix his attention first upon one shock and then upon the other, and the difference, if any, made by this upon the experiment, noted. The shocks were just strong enough to be felt distinctly; but, not so strong as to cause any decided muscular twitch. Before conducting the experiment proper the two induction coils were so adjusted that the shocks were felt as of approximately the same strength.

When the second stimulus was applied half way between  $st.^1$  and  $st.^2$  at the point marked  $st.^a$  in Fig. 5, the method was of course, the same.

#### V. EXAMINATION AND EXPLANATION OF THE DATA OBTAINED

The experiments were started in June, 1911, first in the quiet of the country, and afterwards in the Department of Psychology of Columbia University. The uniformity of the results obtained would seem to prove that individual differences are slight if present at all. As examples we give below a few of the results taken from the notes at random.

"September 26, 10:30 A. M., temperature 62, subject Q. S. (clock maker). At  $1/20$  of a second apart  $st.^1$  felt distinctly first. At  $1/35$  of a second apart  $st.^1$  and  $st.^2$  felt together.

October 3, 10:30 A. M., temperature 62, subject W. C. T. (gardener).  $St.^1$  and  $st.^2$  felt together at  $1/40$  of a second apart. At  $1/40$  of a second apart  $st.^1$  before  $st.^a$ , and  $st.^1$  and  $st.^a$  together at  $1/80$  to  $1/90$  of a second apart.

October 4, 10:30 A. M., temperature 62, subject J. G. (Swiss butler). At  $1/90$  of a second apart  $st.^2$  first, at  $1/45$  of a second apart  $st.^1$  and  $st.^2$  together; at  $1/45$  sec. apart  $st.^1$  before  $st.^a$ ; at  $1/90$  of a second apart  $st.^1$  and  $st.^a$  together.

<sup>1</sup> I. e.,  $st.^1$  given  $1/20$  of a second before  $st.^2$ .

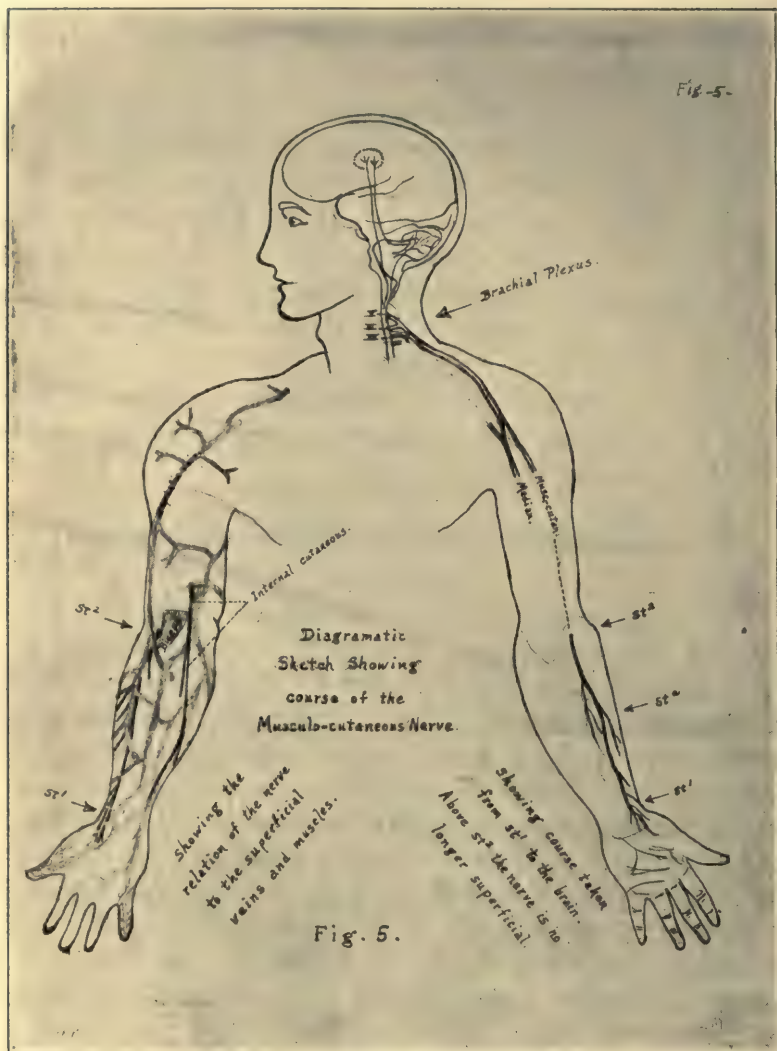


FIG. 5.

February 18, 9:00 P. M., subject A. E. R. (psychologist). At  $1/80$  of a second apart st.<sup>2</sup> felt first; at  $1/35$  second apart, st.<sup>1</sup> and st.<sup>2</sup> felt together; at  $1/40$  second apart st.<sup>1</sup> and st.<sup>2</sup> still felt together; at  $1/35$  second apart st.<sup>1</sup> felt before st.<sup>2</sup>; at  $1/35$  second apart st.<sup>1</sup> before st.<sup>a</sup>; at  $1/40$  second apart st.<sup>1</sup> before st.<sup>a</sup>; at  $1/80$  second apart st.<sup>1</sup> and st.<sup>a</sup> felt together.

The tables given on page 325 represent only a small portion of the work performed, but they are sufficient to illustrate our case. As subjects we used various ages and classes of people. In the case of experiments performed at Columbia, the subjects were with but three exceptions, trained observers in experimental psychology, but difference in temperament or intellectual acumen seemed to have little effect.

It will be seen that when st.<sup>1</sup> precedes st.<sup>2</sup> by an interval of  $1/35$  to  $1/45$  of a second, the sensations occur together. For st.<sup>1</sup> and st.<sup>a</sup> to occur together an interval of  $1/70$  to  $1/90$  of a second is necessary.

A factor that should possibly have been considered is the actual distance between the two stimuli in the case of each subject. The differences may have been partly due to the length of arm in the different observers, since it obviously takes longer for the nervous impulse to travel the 10 inches between the two points of stimulation in a six-foot man than the corresponding 7 inches in the arm of a child, or small adult.

The results obtained from the various subjects experimented on agree so closely in the long run that instead of giving a summary of results we think it better to give merely a small but typical set of experiments (Table I.). Table II. is a tabulation of the results obtained from one subject. It will be noted that the averages obtained from this table agree closely with those of Table I. The time-intervals given in columns 3 to 9, inc., are in one thousandths of a second, and indicate the temporal relation of the sensations when the correlative stimuli are applied successively at the time intervals noted. For example, s.<sup>1</sup> was felt before s.<sup>2</sup> when st.<sup>1</sup> was given .0412 ( $\frac{1}{20}$ ) of a second before st.<sup>2</sup>; or again, when st.<sup>2</sup> was



made to follow st.<sup>1</sup> by an interval of .0245 ( $\frac{1}{40}$ ) of a second, s.<sup>1</sup> and s.<sup>2</sup> were felt to occur together. Of course columns 4 and 8 give the longest times (*i. e.*, in an ascending scale) that allowed the one stimulus to be felt before the other;—the next longer time making the sensations ‘fuse.’ In like manner columns 3 and 7 give the *shortest* time.

The results of this experiment as shown by the data given in the above tables, are curious, and are quite notably different from those that we should have been led to expect from our knowledge of previous experiments in reaction-time and from the experiments on the rate of the nervous impulse.

Although our experiment is not for the purpose of determining the rate of the nervous impulse, *per se*, this must be taken into consideration in a critical analysis of the data obtained. Moreover, in the case of this experiment, we consider this important, since our results seem to be at variance with those of previous investigators.

Numerous experiments to ascertain the speed of the nervous impulse have, as was explained in Section I., been carried on for many years, by various methods, and with varying results. Helmholtz as before stated, obtained a rate of about 30 meters per second for the sciatic nerve of the frog; and this rate is even yet generally considered as being approximately correct. Spencer quotes it in his ‘Psychology,’ and Wundt,<sup>1</sup> says: “The rate of the propagation of the current of action in the nerve-fiber coincides with the rate of the stimulus process itself . . . the stimulus wave, at the rate of some 32 meters in one second.” We are not aware, however, of any trustworthy experiment showing a slower rate than 30 meters per second for either man or animal.

The experiments in reaction-time do not, of course, give the rate itself, but they throw considerable light, in a general way, upon the subject, by fixing a maximum limit. Some of the earlier figures for reaction-time in touch are:

Wundt	Hirch	Hankel	Exner
.201	.182	.1546	.1337

<sup>1</sup> Wundt, ‘Principles of Physiological Psychology,’ Titchener’s translation, 1904, p. 80.

TABLE I.

1	2	3	4	5	6	7	8	9	10
Subject	Distance in inches from St. <sup>1</sup> to St. <sup>2</sup>	S. <sup>1</sup> Felt Before S. <sup>2</sup> when Time Interval Was	S. <sup>2</sup> Felt Before S. <sup>1</sup> when Time Interval Was	Time Interval at which S. <sup>1</sup> and S. <sup>2</sup> were Felt as Simultaneous	Individual Deviation for Column 5	S. <sup>1</sup> Felt Before S. <sup>2</sup> when Time Interval Was	S. <sup>2</sup> Felt Before S. <sup>1</sup> when Time Interval Was	Time Interval at which S. <sup>1</sup> and S. <sup>2</sup> were Felt as Simultaneous	Individual Deviation for Column 9
H. E.	8	.0412	.0095	.0245	-.0018	.0204		.0125	-.0004
F. B.	8	.0500	.0055	.0286	.0023				
F. S.	7½	.0500	.0083	.0333	.0070				
W. C.	8	.0411	.0080	.0250	-.0013	.0250	.0083	.0118	-.0011
J. G.	8	.0444	.0111	.0222	-.0041	.0222	.0083	.0111	-.0018
L. W.	8	.0500	.0080	.0250	-.0013	.0222	.0083	.0118	-.0011
K. H.	7½	.0454	.0111	.0250	-.0013	.0250	.0083	.0125	-.0004
E. E.	7½	.0500	.0083	.0236	-.0027	.0222		.0111	-.0018
M. I.	7	.0500	.0083	.0333	.0070	.0333	.0083	.0166	.0037
B. W.	7	.0500	.0166	.0333	.0070	.0333	.0083	.0166	.0037
A. R.	8½	.0450	.0083	.0277	.0014	.0138	.0083	.0111	-.0018
A. R.	8½	.0500	.0055	.0250	-.0013	.0166	.0055	.0138	.0009
A. R.	8½	.0486	.0090	.0222	-.0041	.0192	.0083	.0166	.0037
F. G.	8	.0338	.0111	.0222	-.0041	.0222	.0055	.0111	-.0018
F. G.	8	.0454	.0100	.0333	.0070	.0192	.0055	.0138	.0009
F. G.	7½	.0285	.0083	.0305	.0042	.0166	.0083	.0166	.0037
D. L.	8½	.0555	.0083	.0250	-.0013	.0192	.0055	.0138	.0009
D. L.	8½	.0444	.0138	.0222	-.0041	.0250	.0028	.0111	-.0018
D. L.	8½	.0472	.0083	.0192	-.0071	.0222	.0055	.0083	-.0046
Sum..		.8705	.1173	.5011	.0704	.3776	.1050	.2202	.0341
Aver..		.0458	.0094	.0263	.0037	.0222	.0070	.0129	.0020

TABLE II.

1	2	3	4	5	6	7	8	9	10
Subject	Distance in inches from St. <sup>1</sup> to St. <sup>2</sup>	S. <sup>1</sup> Felt Before S. <sup>2</sup> when Time Interval Was	S. <sup>2</sup> Felt Before S. <sup>1</sup> when Time Interval Was	Time Interval at which S. <sup>1</sup> and S. <sup>2</sup> were Felt as Simultaneous	Individual Deviation for Column 5	S. <sup>1</sup> Felt Before S. <sup>2</sup> when Time Interval Was	S. <sup>2</sup> Felt Before S. <sup>1</sup> when Time Interval Was	Time Interval at which S. <sup>1</sup> and S. <sup>2</sup> were Felt as Simultaneous	Individual Deviation for Column 9
H.L.E.	8	.0454	.0083	.0333	.0086				
H.L.E.	"	.0454	.0083	.0166	-.0081				
H.L.E.	"	.0555	.0055	.0222	-.0025				
H.L.E.	"	.0552	.0055	.0333	.0086	.0333	.0083	.0166	.0041
H.L.E.	"	.0442	.0055	.0222	-.0025	.0222	.0083	.0111	-.0014
H.L.E.	"	.0333	.0125	.0222	-.0025	.0166	.0055	.0111	-.0014
H.L.E.	"	.0333	.0111	.0250	.0003	.0166	.0083	.0125	.0000
H.L.E.	"	.0333	.0125	.0250	.0003	.0166	.0083	.0125	.0000
H.L.E.	"	.0333	.0125	.0222	-.0025	.0166	.0083	.0111	-.0014
H.L.E.	"	.0333	.0125	.0250	.0003	.0166	.0083	.0125	.0000
Sum..		.4122	.0942	.2470	.0362	.1385	.0553	.0874	.0083
Aver..		.0412	.0094	.0247	.0036	.0198	.0079	.0125	.0012

A more recent rate gives  $1/10$  second from one hand to the other—a distance of more than 2 meters, so that, even without allowing for the complicated process of association, a rate of at least 20 meters per second must be posited.

Starting with this rough estimate we find, that, even if this minimum measurement of 1,260 inches—or 32 meters—per second is approximately correct, the stimulus wave should take not more than  $1/116$  to  $1/145$  second to travel from point st.<sup>1</sup> to st.<sup>2</sup>—a distance varying from seven to ten inches. According to this, therefore, the stimuli should be given at somewhere near this interval apart, in order to have the two sensations occur at the same time, *i. e.*, fuse in consciousness, but experiments show that in order to feel the two shocks as occurring simultaneously, the stimuli must be given from  $1/35$  to  $1/45$ <sup>1</sup> second apart—a time interval three times as great.

What is the cause of this apparent discrepancy? Does it exist merely because of the method used or does the explanation lie deeper?

## VI. POSSIBLE SOURCES OF ERROR

As in all such experiments, the possible sources of error are legion. We enumerate below what we consider the most important.

1. Inability of the subject to perceive such minute fractions of a second, due to a fusion of successive nervous or cortical processes.

(a) Fusion at point of stimulation.

(b) Fusion between point of stimulation and cortex.

(c) Fusion at cortex, due to duration of excitement set up by first stimulus.

2. Inaccuracy of observation, even if such small differences can be perceived.

3. Difference in the sensitiveness of the skin between the two points of stimulation.

4. Varying depth of nerve at different points of stimulation.

<sup>1</sup> A fairly constant average is  $1/40$  second.



5. The involving of other nerves as conductors—varying at the points of stimulation.

6. The subject may concentrate his attention on one of the two stimuli, thus putting the tactile corpuscles and nerve ends in a 'hair-trigger' condition, so to speak.

7. First stimulus may seem to occur longer ahead of second stimulus than is really the case, due to 'lag,' or to the monopoly of the subject's attention aroused by first stimulus, thus setting back the apparent time of the second stimulus.

8. Previous experiments may be wrong, and the actual speed of the impulse along the sensory nerves may be much slower than supposed.

9. The first stimulus may alter the excitability or conductivity of the nerve so as to quicken or retard the second stimulus.

## VII. DEFENSE OF ABOVE SOURCES OF ERROR

Taking up the possible sources of error in order, we observe:

1. Sensations occurring at the rate of several hundred a second can be distinguished as separate sensations, although, of course, they cannot be counted. This is true of both auditory and tactile sensations. Shocks  $1/360$  of a second apart can be distinguished as *not* simultaneous.

James quotes Exner as having distinctly heard the double-ness of two successive clicks of a Savarts wheel, and of an electric spark at  $1/500$  of a second apart; also Von Wittich as observing that 'between 1,000 and 2,000 strokes can be felt as discrete by the finger.' Shocks of  $1/240$  apart can be distinctly felt as discrete, and all the observers in the present experiment were easily able to distinguish shocks  $1/120$  of a second apart, as *not* simultaneous.

There cannot, therefore, be any fusion, unless the succeeding stimuli occur at a much more rapid rate than they are given in the present experiment.

And this is still true, even if a slowing up of the impulsive by the first stimulus (st.<sup>1</sup>) should occur in one or more plexi or synapses in its course to the brain, thus allowing that from st.<sup>2</sup> to catch up sufficiently to counteract or obliterate the spacial distance between the two impulses.

For were such a spacial contraction to take place to any extent (as, for example, the fusion of the two succeeding processes in adjacent neurones), it would be impossible to distinguish between such small intervals as  $1/240$  sec. or  $1/120$  sec. when two similar successive stimuli are applied to the same spot.

Furthermore, the result, as to time, would be the same whether such fusion took place all at once, at the cortex itself, or through a chain of plexi or synapses on the way thither.

But since two succeeding nervous impulses  $1/120$  sec. apart can readily be felt as discrete, they can not fuse either at the cortex or before.

Now as the difference between the generally accepted rate of nervous impulse of  $1/120$  sec. for the distance between st.<sup>1</sup> and st.<sup>2</sup>, and that of  $1/40$  sec. as found experimentally by us is  $1/80$  sec., any condition, either at the cortex itself, or in the course of the afferent nerves, that would obliterate this interval, would, *ipso facto*, obliterate, still more easily, any smaller interval, and make it impossible to distinguish between successive stimuli  $1/80$  sec. apart, or less, which we have found not to be the case. This meets objection 1 under all three headings.

2. The question of inaccuracy of observation does not really enter into this experiment. The point is not whether such minute differences can be distinguished *accurately*, but whether they can be distinguished *at all*. Also, the margin of inaccuracy varies only within certain limits which, in this case, are wide enough to cover the objections. The fact that shocks occurring at much smaller intervals of time apart can be distinguished as *not simultaneous* covers the ground.

3. It is impossible for difference of cutaneous sensitivity at st.<sup>1</sup> and st.<sup>2</sup> to be sufficient to account for such a discrepancy in time. The difference, if any, is only slight. In the course of this experiment, no noticeable difference has been observed.

4. The musculo-cutaneous nerve is at nearly the same depth at both points. It is practically subcutaneous all the way from the st.<sup>1</sup> through st.<sup>a</sup> to st.<sup>2</sup>.

5. Although, roughly speaking, a square inch of surface involves three or four main afferent nerves, in this case any other nerves, that might be involved, are practically the same in length and course, all passing through the brachial plexus on their way to the cortex.<sup>1</sup> Hence, if there be a source of error here, it is practically the same for both points. Experiments, also, always show approximately the same results, even when the electrodes are not accurately adjusted.

6. Actual experiment shows that though attention may have some effect it is slight, if present at all. Attention to either point does not seem to materially vary the result. If attention has any effect, in this experiment, it seems to be limited to a possible apparent varying of *intensity* between the two shocks; not to a change of the time interval.

A typical article on this question is that upon the 'Experimental Research of the Phenomena of Attention,' by James R. Angell and Arthur H. Pierce in *The American Journal of Psychology*, Vol. 4, page 528, following Wundt's experiments. Another is James's criticism of these experiments in his 'Principles of Psychology,' page 415, Vol. 1, in which the possibility of correctly determining simultaneity between the ring of a bell and the position of a pointer were investigated. The results of all similar experiments go to show that there is a complicated margin of error, both positive and negative, due, apparently, primarily to attention, and possibly other factors.

All these experiments, however, seem to have been carried on between different kinds of sensations, as, for example, sight and touch, or series of sensations in rapid succession. This source of error, however, does not seem to apply where only two successive sensations are in question. The remarkable uniformity of our results in the present experiment would appear to be *prima facie* evidence that such a source of error does not play an appreciable part in this case.

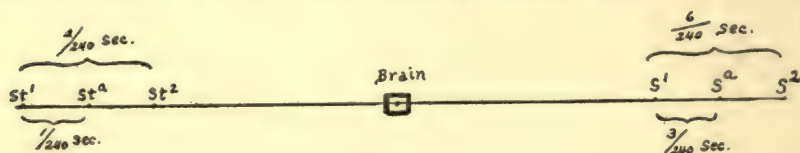
<sup>1</sup> In the case of st.<sup>1</sup> the only other nerve that might by accident receive the stimulus would be a cutaneous branch of the median. On the radial side of the forearm we seldom find branches of the internal cutaneous extending so low as the wrist. In the case of st.<sup>2</sup> the only other nerve in the vicinity is the musculo-spiral. We can often tell if the electrode is in contact with a filament of this nerve by observing the nature of the muscular contraction when the stimulus is increased.



It is well known to the trained musician that the very minute fraction of a second between two tones on the same instruments (*e. g.*, two violins), where these two tones are supposed to be simultaneous, can be easily determined, particularly in some of the older music, like Bach, where violin runs of fifty or sixty notes a second, are by no means uncommon, and must be performed with the greatest accuracy.<sup>1</sup>

7. This monopoly of the observer's attention seems, at first, a serious objection, and a possible source of error undoubtedly here exists. Two things, however, may be said by way of defence. In the first place there is no doubt but that the error is minimized when only *two* successive stimuli are used, since nearly all of the experiments of a similiar nature have been performed with *series* of rapid stimuli.

In the second place, such an apparent "lag" of the second sensation behind the first cannot be great enough, in the present instance, to invalidate seriously the results of this experiment, as will appear from the following diagram.



For, if the actual time between  $st.^1$  and  $st.^2$  is  $2/240$  second (nerve rate, being taken at 32 meters per second), and yet the difference in time between resultant sensations  $s.^1$  and  $s.^2$  is  $6/240$  second, this "lag" would be  $4/240$  second. Therefore, if the actual time between  $st.^1$  and  $st.^2$  is  $1/240$  second, the apparent difference between sensations  $s.^1$  and  $s.^2$  should be  $5/240$  second ( $1/240 + 4/240$ ), since the "lag" would be practically the same in both cases. Whereas, by experiment, we get apparent time  $st.^1 - st.^2 = 3/240$  second, which shows conclusively, not only that "lag" alone cannot account for the

<sup>1</sup> As a matter of fact, no difference in results due to varying attention was found in this case; and we understand that, although Prof. Münsterberg found a difference in the apparent strength of different shocks due to attention, more recent experiments carried on at Columbia have not tended to confirm his results.

discrepancy, but that "lag," in this instance, hardly affects the experiment at all. The classical "complication experiment" does not enter here since we are not using different kinds of stimuli.

8. Even if the minimum rate, usually given, of 30 meters a second is incorrect, it could not be so slow as 10 meters a second, which is what the present experiment would indicate.

The ordinary reaction experiment gives an average of  $2/10$  second for time from stimulus to conscious reaction ( $1/10$  from hand to hand is a minimum). During this time, says James, "(1) The stimulus excites the peripheral sense-organ adequately for a current to pass into the sensory nerve; (2) The sensory nerve is traversed; (3) The transformation or reflexion of the sensory into a motor current occurs in the centers; (4) The spinal cord and motor nerves are traversed; (5) The motor current excites the muscle to the contracting point."<sup>1</sup> Besides all of which, as a sixth point, 'the central elements offer incomparably more resistance than the nerve fibers to the progress of an excitation . . . a retardation of conduction, amounting on the average to 0.003 second, occurs in the spinal ganglia of the frog.'<sup>2</sup> In the light of all that is now known on the subject we feel safe in saying that the 'nerve-current' in all probability moves at its greatest speed over the comparatively unimpeded nerves before the medulla and complicated ramification and decussations of the brain are reached. Where delay exists it undoubtedly occurs in the gray matter and may conceivably be due to the slower conduction in the minute conducting elements such as the dendrites, perikarya, arborizations, etc. Wherever one neurone ends and another starts, *i. e.*, wherever a synapse occurs, delay may be theoretically inferred, but it is extremely improbable that any greater number of synapses are involved when the stimulus is given at st.<sup>1</sup> than when it is given at st.<sup>2</sup>

As a result of an extended series of reaction experiments Cattell and Dolley say, 'we believe—that the velocity is greater than 30 m. per sec.'<sup>3</sup> and again, in the PSYCHO-

<sup>1</sup> James, 'Psychology,' Vol. I, p. 88.

<sup>2</sup> Wundt, 'Physiological Psychology,' Titchener's translation, Vol. I, p. 88.

<sup>3</sup> Cattell and Dolley, 'On Reaction-times and the Velocity of the Nervous Impulse,' National Academy of Sciences, Vol. 7.

LOGICAL REVIEW for March, 1894, page 167, Cattell reaches the same conclusion finding 'a velocity of the nervous impulse in the sensory tracks of the spinal cord of about 40 m. per sec.' Several of the more recent investigators have found even a more rapid rate.<sup>1</sup>

The 'nerve-current' therefore, must move at a much greater rate than ten yards a second ( $1/40$  second for the eight inches between st.<sup>1</sup> and st.<sup>2</sup>), or it could never reach the cortex, pass through all the retardations and transformations enumerated above, and get back to the hand during the time that the reaction experiments show.

It is evident therefore that some other explanation must be given for the apparent discrepancy.

9. That the passage of an electrical current through a nerve causes profound changes in both its excitability and in its conductivity is a conceded phenomenon. The condition is known as *electrotonus*. In view of the fact that a similar condition is caused by a simple electric shock, it is possible that the explanation lies here. It should be borne in mind, however, that two excitatory states recurring at  $1/2,000$  sec. interval can be propagated along the nerve without blending, *i. e.*, the upset of nerve equilibrium by the first stimulus does not interfere with the production of a fresh disturbance by a second stimulus even though only  $1/2,000$  of a second has elapsed. It is quite possible, that the altered condition of the nerve is sufficiently profound and lasts long enough to cause a retardation of the second stimulus, but the same could be said if the interval were  $1/120$  instead of  $1/40$  second. An overlapping is of course possible, but the question is *how far* the response to a second stimulus is prohibited because a previous stimulus is still in progress, or because the nerve has not yet 'recovered.'

<sup>1</sup> Hermann, 'Handbuch der Physiol.,' Vol. 1, Pt. 1, p. 23; Kiesow, 1903, *Ziet. fur Ph.*, Vol. 33, 444; Piper, 1908, *Pfluger's Archiv*, 1908, Vol. 124, p. 591; Chauveau, 'Vitesse de Propagation des Excitations dans les moteurs Nerfs,' etc., 40-75, *Compt. rend. Acad. de Sc. Par.*, 1878, 87, 95, 99, 138-142; Helmholtz and Baxt., 65 M., *Monatsber. der Berliner Acad.*, 1870, 3, 184; Bloch, *Arch. d. Physiol. norm. et path.*, 1875 (Bloch places a rate of 192 m. per sec. in the nerves, and 194 in the spinal cord.) Richet, 'Physiologie des Muscles et des Nerfs,' 1882; Hall and Kries, *Archiv fur Anat. und Physiol.*, 1879, Sup. 1-10; Exner, 'Entwurf z. einer physiologisch Erklärung, 1894.



## VIII. GENERAL ARGUMENT AND DISCUSSION OF RESULTS

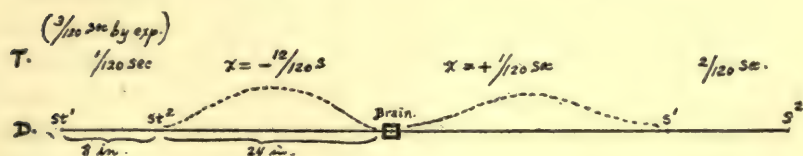
If the results of the experiment described in the foregoing pages are correct, and if no error exists in the apparatus used, we are faced with the apparently anomalous fact that, in order to obtain simultaneous sensations from the two stimulation points in question, the stimuli must be applied at such intervals as to cause them to reach the appropriate cortical center in the neighborhood of  $1/60$  second apart. We may therefore conclude that:—either such short time intervals cannot be distinguished, which we have shown is not the case; or the sensations (or whatever immediately gives rise to them) do not occur simultaneously in the brain at all. Yet the processes must occur practically together *somewhere*, in order to enable the concomitant sensations to occur together. Where they occur is another question.

Again, there must exist *some time interval* between the cortical process (the brain state) and the occurrence of the allied psychic process (the sensation) since it is not probable that one sensation can be synchronous with the cortical process, and the ensuing sensation come after it.

A time interval in one case necessarily presupposes a time interval in the other case.

And if so, what is the relation between them?

The following diagram illustrates the case graphically.



Time  $st.^1 - st.^2 = 1/120$  sec. (30 yards a second).

Time  $st.^2 - B(x)$  less than  $12/120$  ( $1/10$ ) second, since reaction experiment shows at most  $2/10$  for  $st.^2 - B - st.^2$ .

But difference of  $st.^1 - st.^2 = 3/120$  second ( $1/40$ ). Therefore  $s.^1$  and  $s.^2$  must occur synchronously at some such place as  $s.^1$  or  $s.^2$ .

The same holds true when  $st.^a$  is applied at half the distance between  $st.^1$  and  $st.^2$ , giving the same proportionate result.

It will be observed that time  $st. - B$  is unknown ( $x$ ), except that it is, presumably, not more than  $1/10$  second, since the ordinary reaction experiment gives  $2/10$  second as the average time. In the same way, Time  $B - S$  is unknown. One of the main things, therefore, in this experiment, is to discover whether such a time interval exists at all.

The foregoing experiment would seem to indicate, not only that such a time interval *does* exist between the cortical process and the ensuing sensation, but that the interval is probably of a measurable duration. In the case of a sensation, arising from a stimulus applied at the wrist, the time that elapses between the arrival of the stimulus-wave at the cortex and the following sensation would seem to be in the neighborhood of  $1/60$  second—or if the ample margin of one half of the apparent interval be allowed for the possible sources of error,  $1/120$  second—an interval by no means so small as to be imperceptible or unmeasurable.

## IX. CONCLUSION

In summing up the results of this investigation we desire to again call attention to the fact that the experiment is not one for the determination of the speed of the nervous impulse, *per se*, for we are not concerned as to whether the speed is 20 or 200 meters per second. Had we been desirous of knowing the exact speed, and of *using* the same in our computations, a different apparatus would have been necessary.

The more probable of the many sources of error that are necessarily entailed in an experiment of this nature have been discussed in Section VI. These possible errors were discussed and defended to the best of our ability in Section VII. but the authors make no claim that their “defense” is conclusive. They realize that not only should the length of nerve traversed be considered,—but the number of synapses as well, and they further realize that even though the number of synapses be the same, and even though the sensitivity of the two points chosen be equal, that the *frequency* with which the nerve is accustomed to transmit tactile sensations is a most important factor.

In this connection we desire to make mention of a factor that was not considered in Section VI., since we do not consider it a source of error in any sense of the word. It is one, however, that needs consideration; we refer to the fact that with certain nerves, only *certain kinds* of stimuli will elicit a response. Many instances can be cited in which particular reflexes can be elicited only by certain stimuli. A very characteristic reflex in the cat is the pinna-reflex. When the pinna is tickled or touched in a certain way it twists so that its free end is turned backward. This reflex is elicitable by various *mechanical* stimuli but it is impossible to provoke it by any form of *electrical* stimulation.<sup>1</sup> Here, as in the case of the reflex-arc, and notably in the organs of *sense*, it is probable that the terminal nerves and tactile corpuscles are "attuned," so to speak, to respond only to stimuli of a certain definite kind. We may say that the chief function of the 'organ' or 'receptor' in which the reaction starts is to convey to the central nervous system sensations of one kind, and of one kind only, and it is the function of the 'receptor,' be it a Pacinian corpuscle, an end-bulb of Krause, or a plain tactile corpuscle, not only to lower the threshold of excitability for a certain type of stimulus but to heighten it for all other types, these stimuli not being such as the mechanism is adapted for. We think it extremely improbable, however, that the musculo-cutaneous nerve is any better adapted for electrical stimuli at the elbow than it is at the wrist. If there is any difference it must be one of degree and not of kind, but experiment would seem to prove that there is but little difference in the tactile sensitivity of the two points.

The authors realize that the possible error No. 7 given in Section VI. is a serious one, and that it is one, which, in consideration with the relative use of the parts, *i. e.*, the *frequency with which the nerves are accustomed to transmit tactile and other stimuli*, needs further consideration. But whether the 'apparent discrepancy' is explainable here, or elsewhere, the experiment, unless the apparatus is at fault, offers a contribution, small though it may be, to the laws underlying nervous activity.

<sup>1</sup> Sherrington, 'The Integrative Action of the Nervous System,' page 10.



Finally, unless a purely mechanical or physiological explanation is offered, we offer a possible contribution to the doctrine of psychophysical parallelism. This has generally been left to the armchair-philosopher, and thus far the experimental psychologist has contributed little if anything. The experiment that it is the object of this paper to present, can, however, here offer something that may prove suggestive. For we are confronted with the seemingly anomalous fact that in order to get two sensations to 'fuse' they have to be applied at an interval of about  $1/40$  of a second, whereas according to the current theory the interval should not have to be even one third of this. It would seem that whatever the relation of cortical process and corresponding sensation may be, it is not one of simultaneity, and speaking temporally we might even say that  $s.^1$  and  $s.^2$  do not occur together in the brain when they are felt as synchronous.

In conclusion we wish to say that although the method of conducting the experiment, as described in Section IV. was, at the time, considered the best possible, we have come to realize that what is known as the "method of right and wrong cases" would have been better. It is the one that we shall use in the future, either alone or in conjunction with others. With this method we shall so *fix* the disks that shock  $st.^2$  will follow shock  $st.^1$  after an interval that will be too small to be noticed each and every time, but not so small that it will not be noticed *most* of the time. Thus with this method we can ascertain by the percentage of right judgments the subject's degree of perceptibility. Having found the percentage of right cases given by several subjects with the contacts separated at a certain distance, we could then compute from this, the distance that the contacts should be separated in order to obtain any percentage desired of right cases. We propose a solution of this rather intricate problem in the near future, by a combination of various methods, and by using tactile as well as electrical stimuli.

# THE PSYCHOLOGICAL REVIEW

---

## INTROSPECTIVE ANALYSIS OF CERTAIN TACTUAL PHENOMENA<sup>1</sup>

BY GEORGE F. ARPS

*Ohio State University*

The introspections of this paper were obtained from two groups of pressure stimuli. In the first group the pressures were more or less extended; in the second group they were of momentary duration.

### I. TACTUAL STIMULI OF MORE OR LESS EXTENDED DURATION

The data forming the basis of part of this paper were gathered in the laboratory at Leipzig, and some of these appeared in the November, 1908, *Psychologische Studien*.<sup>2</sup> The data under consideration here, being an integral part of that already published, were obtained under precisely the same conditions as given in the above-mentioned publication. A brief statement of these conditions follows:

Two tactual stimuli, a normal and a comparative, were applied successively to the upper phalanges of the index and middle fingers. The normal stimulus remained constant in intensity and varied in duration; the comparative stimulus, on the other hand, remained constant in duration and varied in intensity. The method of minimal changes was employed.

Two norms, 134.2 gr. and 58.2 gr. respectively, were used. Each of these norms had the following duration periods: 13°, 45°, 72°, 121°, 206°, 305°, 380°, 432°, 487°, 611°, 980°, 1,385°

<sup>1</sup> A paper read before the American Psychological Association at the Minneapolis meeting, December, 1910.

<sup>2</sup> Wundt, *Psychologische Studien*, Vol. IV., pp. 431-471.

TABLE I

Duration of N.-S. 134.2 Gm.	$13^{\sigma}$	$45^{\sigma}$	$72^{\sigma}$	$121^{\sigma}$	$206^{\sigma}$	$305^{\sigma}$
Series of C.-S. in grams. Duration one sec.	10 (I) 15 20 (I') 25 30 35 40(1')	30 (I) 35 (II'') 40 (I') 45 50 55(2'') 60(1')	40 (I'') 45 (III) 50 55 (I) 60(1') 65(2') 70(3) 75 80	(II') 50 (I+I) 55 (V) 60 (I'') 65 70(5+3'') 75(3')	60 (II'') 70 (III+II+I') 80 90(1') 100(1') 110(3'+4'+5) 120(1)	(I') 65 75 (III'+III) 85 95(3) 105(2''+3') 115(2) 125
Duration of N.-S. 134.2 Gm.	$380^{\sigma}$	$432^{\sigma}$	$487^{\sigma}$	$611^{\sigma}$	$980^{\sigma}$	$1,385^{\sigma}$
Series of C.-S. in grams. Duration one sec.	70 (IV'') 80 (I') 90 (III') 100 110(2'+7) 120 130(1) 135	65 75 85 (I'') 85 (V'') 95(1) (I') 105(4'+8+1'') 115 125(2')	(II') 75 85 (IV'') 95 (II) 105 115(5''+5') 125(2'') 135(1)	80 90 (II) 100 (IV) 110 120(5) 130(2) 140(2')	(I') 90 110 (II'') 130(3'') 150(4') 170(1')	85 (II') 105 125 (II+III') 145(1') 165(5') 185(3') 205



TABLE I—Continued.

Duration of N.-S. 134.2 Gm.	13°	45°	72°	121°	205°	305°
Series of C.-S. in grams. Duration one-half second.	40 50 60 70 80(1) 90 100	50 60 70 80 90(2°) 100 110	60 70 (1) 80(1) 90(2') 100 110(1) 120 130	(II°) 60 80(2) 100(2'+1'') 120 140 160	60 (III') 80 100 (II'') 120(1) 140(1°+2) 160(1) 180	70 (II) 90(1) 110(2'') (II°+1) 130(2+1°) (II') 150(1°+1) 170 190
Duration of N.-S. 134.2 Gm.	380°	432°	487°	611°	980°	1,385°
Series of C.-S. in grams. Duration one-half second.	(II°) 70(2°) 90 110(5') (IV) 130 150 (I°) 170(2°) 190 210	60 80 (I') 100(3) 110 (I''+II') 120(1'+1'') (IV'') 140(2) 160(2) 180	(I') 80 100 (III°) 110 120(3+1') 140 (I) 160(2) 180	70 90(1'') 110 130(2°+3) (I) 150 170 190(1°)	70 (II°) 90 110(2') 130 (II+I°) 150(2'') 170 190(1°+1) 210	60 80 (III'') 100(1) 120(3) (I') 140 160(2+1°) 180 200

The measuring, or comparative, stimulus had for one set of experiments a duration of one second; for a second set, one half second. The norms were at one time measured in terms of a comparative stimulus enduring for one second, at another time in terms of a comparative stimulus enduring for one half second, for each of their duration-periods.

The recurring complaint of the variable nature of the normal stimulus caused the writer to look carefully after the source of these apparent variances. All observers were asked to report any changes observed in the norm. Each change was properly noted in the series. That the phenomenon is not due to variations in the objective factors is shown by the carefully made tests at the time.<sup>1</sup> The explanation of the oscillatory nature of the norm is, therefore, to be sought in the subjective realm. An actual case will illustrate.

If a normal pressure of 134.2 gr. persists for a period of 380 sigmas and we measure its intensity in terms of a second pressure persisting for one second and if we use in this measurement a series of pressures of 70, 80, 90, 100, 110, 120, 130, 135, grams respectively, we observe that the norm when measured by 80 grams appears less intense than when the measurement has extended to a comparative stimulus of 110 grams or 130 grams. At 135 grams no assimilative effect is recorded. Frequently observer Franken maintained a gradual increment in intensity of the norm, paralleling the increment in the comparative series. In the above series, the norm subjectively increased as the comparative stimulus actually increased. What is true of an ascending series of intensities is, to a less degree, true of a descending one, in which latter case, 80 grams represents the lower terminus of assimilative effects. Contrast effects become manifest at the termini of the ascending and descending series when the intermediate members, 90, 100, 110, 120 grams are omitted. A summary may be had from Table I.

	(Norm 1st)	(Norm on index finger)
I = One	change reported in 1st time-order,	1st space-order, descending series.
	(Norm 2d)	
II' = Two	changes reported in 2d time-order,	1st space-order, descending series.
		(Norm on middle finger)

<sup>1</sup> Arps, 'Über den Anstieg der Druckempfindungen,' *Psychologische Studien*, Vol. IV., p. 467.

I° = One change reported in 1st time-order,	2d space-order, descending series.
III'' = Three changes reported in 2d time-order,	2d space-order, descending series.
I = One change reported in 1st time-order,	1st space-order, ascending series.
2' = Two changes reported in 2d time-order,	1st space-order, ascending series.
I° = One change reported in 1st time-order,	2d space-order, ascending series.
3'' = Three changes reported in 2d time-order,	2d space-order, ascending series.

The underlined figures represent the approximate positions, in the series, where the assimilative effects were observed to cease. No reports of norm variations were given above or below these points. It is extremely difficult to state definitely the exact points where the "attraction" between the two stimuli begins or ceases. Theoretically, it may be said to be coequal in extent with the series of comparative stimuli. The small arabic figures, enclosed in parentheses to the right of the tabular figures, signify the number of times and at what point in the ascending series, variability in the norm was reported. The Roman numerals to the left have a similar significance for a descending series. For a norm enduring 13 sigmas, the observer reported two changes when the comparative stimuli were given in the descending order (from 45 to 10 grams) and one change when given in the ascending series (from 10 to 45 grams).

It will be observed that the effect of one stimulus upon the other was confined to an upper and lower limit, above and below which assimilative effects ceased.

The number of introspections indicates that the greatest variability in the normal stimulus lies between the time periods 121 and 487 sigmas inclusive; it reaches its maximum at 432 sigmas. A very wide difference in the intensity of the two comparable stimuli is unfavorable to norm variability. On the other hand, a gradual increment of measuring stimulus, by a subtle insinuation, appears to draw the norm above or below its real intensity.

#### DEDUCTIONS FROM THE TABLES

1. It appears that the comparative stimuli under the given conditions render constancy in intensity in a given norm subjectively improbable.
2. The assimilating elements (comparative stimuli) are most



efficient within certain limits of a series, above and below which assimilative effects are minimal or entirely lacking.

3. Greater assimilative effectiveness is found when the normal stimulus precedes the comparative stimulus (1st time-order). This is true for both ascending and descending series of comparative stimuli for the second and half-second periods of duration. Out of a total of 299 apparent norm variations, 190 were noted when the normal stimulus preceded the comparative stimulus (1st time-order) and 109 variations when the normal stimulus followed the comparative (2d time-order). It is probable that this is attributable to the fact that in the first time-order the assimilating element is an immediately present sensation, and, therefore, more efficient than the assimilated element (normal stimulus) which in this time-order is a memory element.

#### DEDUCTIONS FROM CURVES

1. There is a noticeable increment in assimilative efficiency when the comparative stimuli are given in the ascending series. That is, it seems more difficult to secure a subjective decrease in the intensity of the norm than a subjective increase. A total of 120 changes are noted in the descending series to 179 in the ascending series.

2. It appears that the assimilative efficiency, within certain limits, is proportional to the duration of the norm and of the comparative stimulus. For the normal stimulus the optimal period of duration for maximal efficiency lies between 121 and 487 sigmas; for the comparative stimulus the optimal period is one second.

3. It appears that some momentum on the part of the comparative stimuli is necessary to induce assimilation. This is evidenced from the fact that changes for the ascending series are more numerous at the upper limit of the series, while those for the reverse series are more numerous at the lower limit.

Ordinates indicate the number of norm variations; abscissæ indicate norm durations.

I. ----- Curve of norm fluctuations accompanying a descending series of intensities. (C.S. 1 sec.)

- III. ————— Curve of norm fluctuations accompanying a descending series of intensities. (C.S.  $\frac{1}{2}$  sec.)
- II. ————— Curve of norm fluctuations accompanying an ascending series of intensities. (C.S. 1 sec.)
- IV. ————— Curve of norm fluctuations accompanying an ascending series of intensities. (C.S.  $\frac{1}{2}$  sec.)

What are the influences at the bottom of this illusion? It is important to note here that the observers, without exception, regarded the variable comparative stimulus with much keener interest than that of the norm. It is not unlikely that the perception of change in the comparative stimulus grafted itself to the norm, and was a contributing factor in the illusion. In the normal procedure of the experiments a change in the intensity of the comparative stimulus was expected. Each observer was apprised of the intensity-variation of the measuring stimulus and the time-variation of the norm. It is not only probable that a definite perception of changes in the one stimulus affects the other, but that the *expectancy* of change, in which attitude the observer finds himself, plays a part in the illusion, determining the tendency of the judgment.<sup>1</sup>

Assimilative effects of a somewhat different order are those pertaining to the duration of the comparative stimulus, which stimulus, in the opinion of all observers, varied in its duration. Numerous tests were made which showed that the objective factors were constant and that, therefore, the fluctuations must be subjective in origin. It is believed here that the *normal stimulus acts assimilatively on the comparative stimulus with reference to its duration, as does the comparative stimulus on the normal stimulus with reference to its intensity.*

The amount of experimentation is insufficient on this point. However, the records show a sufficient number of statements of variation in the time of the comparative stimulus to merit attention. Comparatively few variations are recorded for the periods preceding 12I and following 6II sigmas. When the normal pressure persists for 6II sigmas the time variations are most emphatic. It is doubtful whether the factors given as causing the intensity illusion on the part of the norm apply equally to the time illusion on the part of the comparative stimulus.

<sup>1</sup> Arps, 'Über den Anstieg der Druckempfindungen,' Wundt, 'Studien,' Vol. IV.

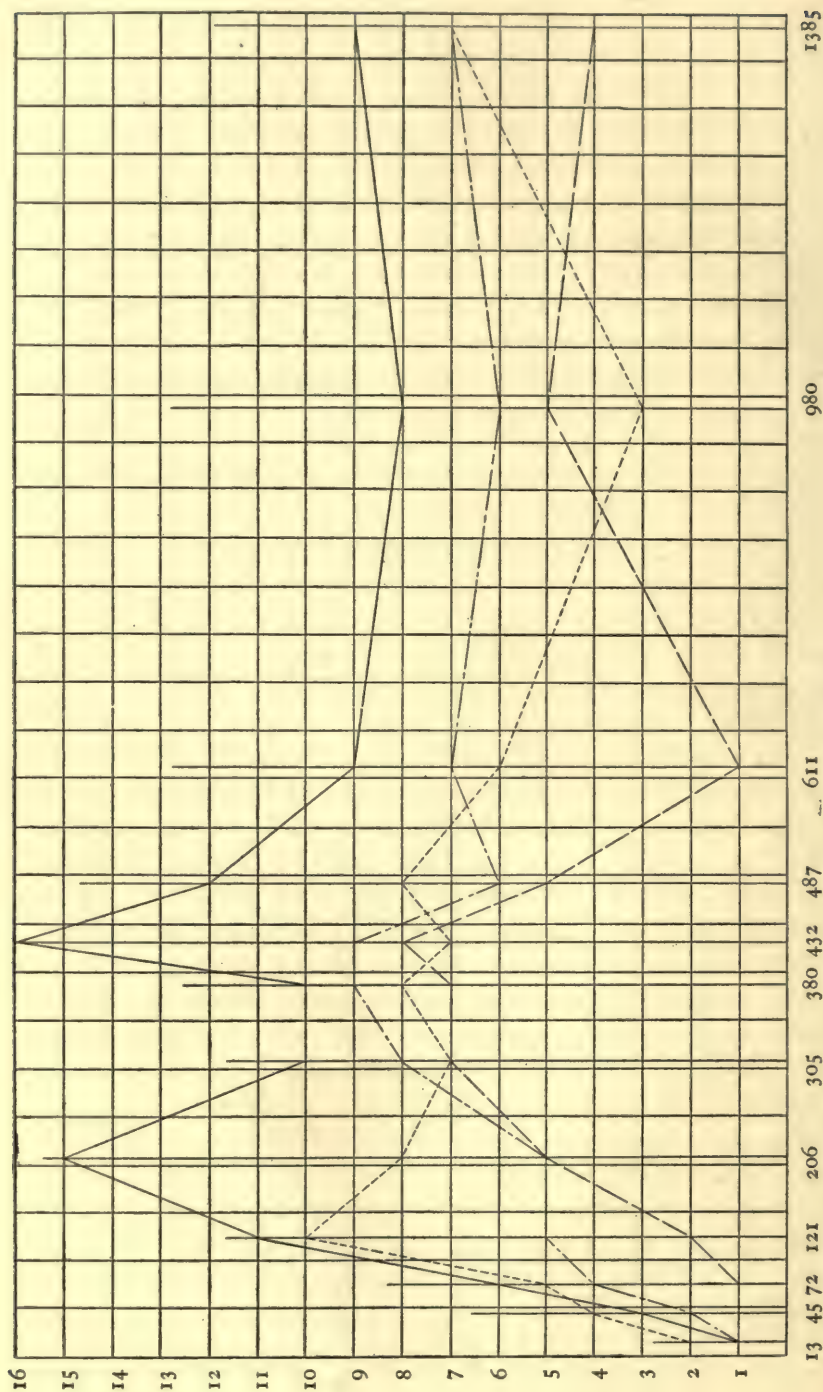


FIG. 1.



The associated elements in this illusion have a somewhat different character from those involved in the illusion of intensity mentioned above. In the latter case are found only like or homogeneous attributes of intensity; in the former case are found the unlike attributes of intensity and duration. These unlike attributes are, however, of the same sense department, namely—pressure.

In just what manner the intensity of the pressure element associates itself with the attribute of duration is not clear. We may have here an intermediate form of assimilation—an association of only partially like elements. It is clear that the assimilation between the attributes of duration and intensity is not identical with the assimilation between the intensity of one pressure and that of another of the same kind. In the latter case the attributes are homogeneous throughout.

The work of Dr. Otto Klemm is interesting in this connection. He investigated the assimilative effect of light and sound. His method consisted in placing a tone stimulus in the median-plane of the observer about four feet distant and a light stimulus either to the right or to the left of the tone stimulus. It was so arranged that a tone of a given duration and intensity could be given successively or simultaneously with the light. Among the results the following are mentioned: (A) The light appears to draw the tone out of the median-plane, right or left, according to whether the light is placed to the right or left of the tone. (B) Increasing the distance of the light beyond a certain point causes a cessation of the assimilation. We observe here that assimilation is effective between disparate elements within certain limits. If we transgress beyond the above limits of assimilative effectiveness, we enter the field of contrast.

Another form of illusion of intensity is observed in the inclination of several observers to render a judgment before the close of the normal pressure (when the norm was given second) even before the beginning of it. Observer Franken says in this connection: "I carry the comparative stimulus over to the norm. Often a judgment is already formed upon the completion of the comparative stimulus. This judgment

is then either confirmed or rejected by the immediately succeeding normal pressure. It is usually confirmed. Should the normal pressure appear other than expected, the final decision is linked with a feeling of surprise and disappointment."

It appears that the absolute impression of the normal stimulus is especially potent in the comparison of the two stimuli. In an act of comparison as here described, factors, other than the sensation connected with the two pressures, enter.

There are a number of confirmatory investigations made along this line which point definitely to the presence of further conscious factors which aid in the formation of a judgment. The investigations of G. E. Müller and Lillian J. Martin,<sup>1</sup> shed some light on the factors involved in such a comparison. Martin and Müller after the method of Right and Wrong cases compare two weights, which are successively lifted. The results show that a judgment is reached in many cases, not on a direct comparison of the two impressions, but to a large extent on the absolute impression of heaviness or of lightness of the weights lifted. How do we gain this knowledge as to whether one of the lifted weights is light or heavy? In much the same way as we arrive at a knowledge in everyday life, whether a letter, a book, a trunk or a child is heavy or light, without comparing this object to some other definite object of the same kind. There is obviously here a factor, other than the immediate impressions of the two weights, which acts as a criterion for a judgment.

Other writers in other fields have observed mediate factors

<sup>1</sup> 'Beiträge zur Analyse der Unterschiedsempfindlichkeit,' pp. 44-45. "Unser Urtheil über die beiden gehobenen Gewichte beruht in vielen Fällen nicht auf einer Art von Vergleichung derselben, sondern stützt sich nur auf den absoluten Eindruck des einen derselben. Und zwar wird unser Urtheil, da es jedes Mal bei oder nach der zweiten Hebung abgegeben wird, selbstverständlich leichter durch den absoluten Eindruck des zuzweitgehobenen Gewichtes bestimmt als durch denjenigen des zuerst gehobenen Gewichtes, der nur durch die Erinnerung auf das Urtheil zu wirken vermag. Macht das zuzweit (zuerst) gehobene Gewicht den absoluten Eindruck der Leichtigkeit, so haben wir eine Tendenz, das zuzweit gehobene Gewicht für kleiner (grösser) zu erklären als das zuerst gehobene; macht das zuzweit (zuerst) gehobene Gewicht den Eindruck der Schwere, so ist eine Tendenz vorhanden, das zuzweit gehobene Gewicht für grösser (kleiner) zu erklären als das zuerst gehobene."

in consciousness which purport to aid the connection between a complex of sensation and its expression in a judgment.

Stumpf and Meyer<sup>1</sup> investigated the sensitivity to discord (*Verstimmungen*). Two tones were given which at one time stood in octave relation, at another time approximated this relationship. The observer was to state whether the interval appeared pure, too large, or too small. Stumpf reached the conclusion that there are elements of consciousness other than the two primary tone sensations, which act as criteria for a judgment. For the lengthened interval such elements as "Unlustgefühl" of tension, "Schärfe," "Überreizung" make themselves felt; for the shortened intervals an "Unlustgefühl der Mattigkeit, Schalheit, Stumpfheit"; and for the subjectively pure intervals, a distinct feeling of pleasure.

From the introspections, it appears that a narrow definition of assimilation is untenable. Such definition cannot be limited, for example, to simultaneous associations or to associations of elements belonging to like compounds. The work of Klemm conclusively shows the assimilative effects between a given stimulus and a disparate one. We may group the various kinds of assimilation here observed into the following classes:

- (1) Between homogeneous attributes, *i. e.*, intensity.
- (2) Between disparate attributes, *i. e.*, intensity and duration.
- (3) Between heterogeneous elements, as illustrated by the assimilation of sensory elements of disparate senses, *i. e.*, visual sensation and auditory sensation.

## II. TACTUAL STIMULI OF MOMENTARY DURATION

The infrequent manifestations of subjective fluctuations in the normal stimulus of extended duration (134.2 grams) for the briefer periods of exposure (13 and 45 sigmas) led to the belief that duration is an important factor in the production of such fluctuations. To this end pressure stimuli of momentary duration were given in a manner similar to the pressure stimuli of extended duration.

<sup>1</sup> *Zeitschrift für Psychologie der Sinnesorgane*, 18, pp. 390-392.



Fig. 2 diagrammatically represents the manner of presenting stimuli of momentary duration. The pendulum,  $p$ , of the

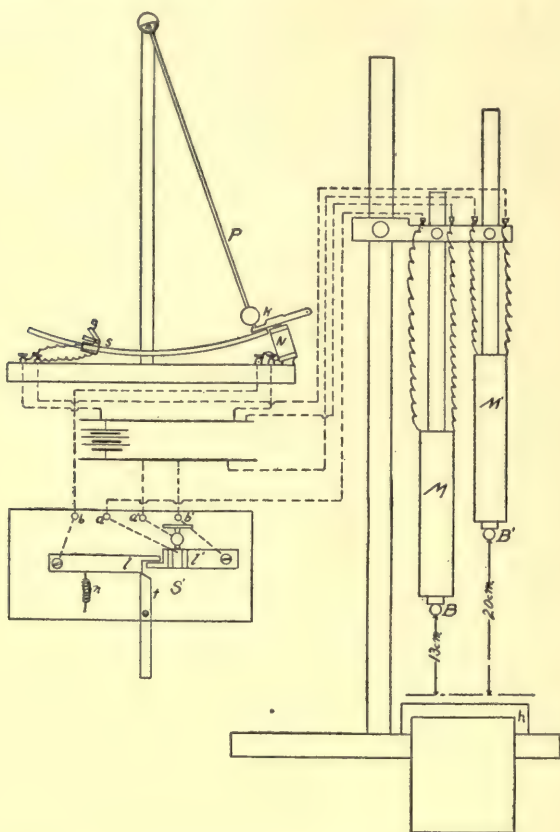


FIG. 2. Apparatus securing momentary pressure stimuli.

Bergström chronoscope is retained by a hook,  $k$ . The switch,  $S'$  (in position shown) is closed between  $a$  and  $a'$  and open between  $b$  and  $b'$ . In this position and with the contact made at  $S$  the electromagnets  $M$  and  $M'$  are receiving current.

The apparatus is set in function by pulling the trigger,  $t$ , to draw the arm,  $l$ , into contact with  $l'$ , by means of the spring,  $n$ , thus connecting  $b$  and  $b'$  and simultaneously breaking connection between  $a$  and  $a'$ . The electromagnet,  $N$ , now actuates the hook,  $k$ , releasing the pendulum and simultaneously break-

ing the current controlling the electromagnet,  $M$ , which releases the suspended ball,  $B$  (normal stimulus). The pendulum swings through its arc opening the switch,  $s$ , by striking the projecting arm,  $g$ . This breaks the current to the electro-magnet,  $M'$ , which releases the ball,  $B'$  (comparative stimulus).

### METHOD

The standard stimulus consists of a small metal ball weighing 3.5 grams. To secure a variety of intensities the ball is dropped perpendicularly from three positions. These positions, from the magnet contact,  $B$ , Fig. 2, to the upper phalanges of the index finger, are 13, 27 and 40 cm. respectively. The right hand is placed, with special reference to these positions upon a rest,  $h$ . Owing to repeated complaints of pain, very few readings were taken from the 40 cm. position. These readings are consequently neglected.

The comparative stimuli, applied so far as practical after the method of minimal changes, consist of a series of eight small metal balls, designated by numbers reading from one to eight inclusive (Table II.). The weight of the various members of the series, following the order designated, is as follows: 1.4-2.2, 3.5, 4.5, 5.8, 7.4, 10.1, 13.6 grams. The distance through which the comparative stimuli drop remains constant at 20 cm. from the magnetic contact  $B'$  to the upper phalanges of the middle finger. The comparative stimulus follows the standard for the first and second position by an interval of 302 and 331 sigmas respectively.

TABLE II

*Normal Stimuli 3.5 Grams*

Variations in Norm First Position (13 cm.) for Increasing and Decreasing Series of Comparative Stimuli	Variations in Norm Second Position (27 cm.) for Increasing and Decreasing Series of Comparative Stimuli	Total Variations for Both Positions for Increasing and Decreasing Series of Comparative Stimuli
(I) 1 (1)	I	(I) 1 (1)
2	(II) 2 (1)	(II) 2 (1)
(II+1) 3	(I) 3	(III+1) 3
4 (2)	4 (1)	4 (3)
5 (1)	[2] 5	[2] 5 (1)
[2] 6	[1] 6 (1)	[3] 6 (1)
[1] 7	7 [1]	(1) 7 (1)
8	8	8

- (1) = One increasing variation observed in the norm when the comparative stimuli are given in an increasing series of intensities.
- (I) = One decreasing variation observed in the norm when the comparative stimuli are given in a decreasing series of intensities.
- (I + 2) = One decreasing and two increasing variations observed in the norm when the comparative stimuli are given in decreasing series of intensities.
- (2 + I) = Two increasing and one decreasing variations observed in the norm when the comparative stimuli are given in an increasing series of intensities.
- [I] = One increasing variation observed in the norm when the comparative stimuli are given in a decreasing series of intensities.
- [I] = One decreasing variation observed in the norm when the comparative stimuli are given in an increasing series of intensities.

The results are practically negative. Only 21 cases of norm fluctuations are recorded among a total of 1,622 experiments. Assimilative efficiency appears dependent upon the *duration* of the presented stimuli. Both assimilative and assimilated elements appear to require more than momentary duration to induce fluctuations in the subjective intensity of the norm. This is first evidenced by the introspections recorded for the normal stimulus of 134.2 grams with a duration period of 13 sigmas when measured by a comparative stimulus enduring for one second (Table I.). The norm in this case approximates momentary duration while the measuring stimulus endures for one second.

In opposition to the view here put forward it may be urged that the method of minimal changes is not sufficiently adhered to to bring about assimilative effects between stimuli of momentary duration. The series of balls composing the comparative stimuli fail to shade by imperceptibly small increments from one of the series to the next above or below it. The minimal obtainable difference between any two consecutive



members of the series is .8 gram; the maximum difference, neglecting the last, is 2.7 grams. The distribution of judgments, however, clearly indicates that the incremental values in the ascending and descending series of comparative stimuli here used in no way impairs the validity of the method employed. That the factor of duration conditions the above phenomenon of assimilation is therefore believed to be valid.

# ESTHETICS OF SIMPLE COLOR ARRANGEMENTS

BY KATE GORDON

*Mt. Holyoke College*

The experiments reported below in part I. of this paper were performed in the psychological laboratory of Mount Holyoke College during the year 1905-6. Those reported in part II. were performed in the psychological laboratory of Columbia University during the year 1906-7. Since the further tests which I wished to make must be indefinitely postponed there seems no reason for delaying any longer the publication of these little studies.

## I

The question which suggested these tests might be phrased as follows. "In massing colors on a canvas is there any general reason for placing certain colors near the center and others near the outside. For example, in combining red with blue may we consider that it is better usually to put the blue in the center and the red in the peripheral parts of the field, or the red in the center and the blue in the periphery?"

Preliminary tests were made with colors arranged as in Fig. 1. A central square of one color was surrounded by four

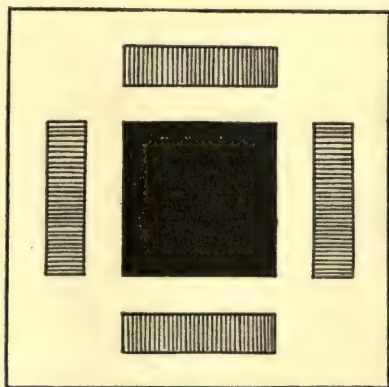
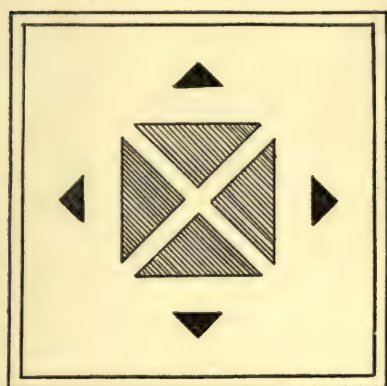
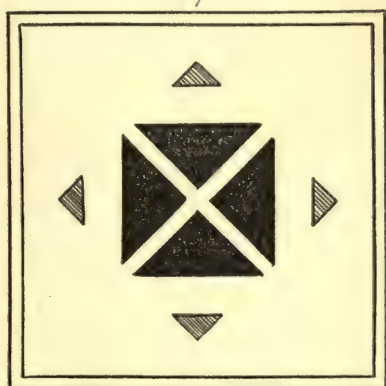


FIG. 1.

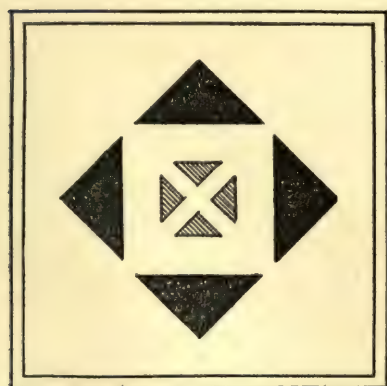
strips of another color, the spatial extent of the two colors being equal. This proved unsatisfactory for two reasons: (1) The figure as a whole was ungraceful and uninteresting to the subjects, and (2) the central color was disliked on account of its unbroken mass. The subjects found their attention repelled by the undifferentiated center, and it was evident that more complexity must be introduced into the figure to get an unquestioned esthetic reaction. The researches of Pierce<sup>1</sup> and Puffer<sup>2</sup> suggested that, since colors of different brightness were



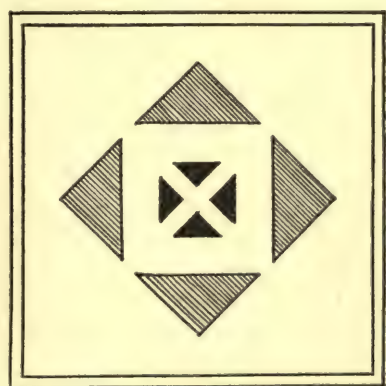
I.



II.



III.



IV.

FIG. 2, I., II., III., IV.

<sup>1</sup> 'Aesthetics of Simple Forms,' *PSY. REV.*, Vol. I.

<sup>2</sup> 'Studies in Symmetry,' *Harvard Studies*, Vol. I.



to be used, a contrast of large and small masses would be desirable. The figures finally chosen, Fig. 2, I., II., III. and IV., seemed the simplest ones which would meet the requirements. These four designs when filled out with a given color combination, *e. g.*, blue and red, represent the following possibilities of arrangement:

I. Small masses of blue in the periphery with large masses of red in the center.

II. Small masses of red in the periphery with large masses of blue in the center.

III. Large masses of blue in the periphery with small masses of red in the center.

IV. Large masses of red in the periphery with small masses of blue in the center.

The experiments were carried on in daylight illumination, and the colors used were the saturated hues of the yellow, green and blue of the Milton Bradley papers and the red of the Hering laid-on discs. The relative brightness of these colors may be stated in terms of the Hering gray paper series. The colors were matched with gray by indirect vision, and it appeared by this method that the

yellow equals in brightness Hering gray No. 2,  
green equals in brightness Hering gray No. 8,  
red equals in brightness Hering gray No. 13,  
blue equals in brightness Hering gray No. 24.

The color combinations in these tests never included more than two colors at a time. This made only the following six combinations possible: blue-yellow; red-green; blue-red; green-yellow; blue-green; and red-yellow. All of these were used.

The method of presenting the combinations by paired comparisons was rejected after some trials. Anyone who has tried it with esthetic tests will recognize, I think, the serious objection against it, that it so quickly exhausts the esthetic reaction.

The figures were shown first upon a background nearly black, made of No. 45 Hering gray paper. This background paper, mounted on cardboard, was surrounded by a black frame 30 cm. square. The subject sat two meters away in

front of a shelf which was draped in gray. Upon the shelf were set, with interspaces of about 6 cm. the four frames containing the different figures. The subjects closed their eyes until the figures were all in place; they were then told to open their eyes and choose the most agreeable of the four designs. No restriction was made on the method of observing the figures, no fixation point was maintained and no time limit set. The subject was also asked to make a second and a third choice. Thus the task was to name the four designs in the order of preference. The frames were then taken down and the four figures filled out with another color combination. These tests were all repeated at a later sitting with this variation, that the figures were shown in reverse order on the shelf, namely 4, 3, 2, 1. Professor Martin<sup>1</sup> has pointed out the importance of relative position in the choice of simple figures, but since these figures were somewhat complicated and had individuality it was thought unnecessary to present them in each possible order, as 2 1, 4, 3; 3, 1, 2, 4, etc.

The subjects were twenty-nine young women in the junior and senior classes of Mount Holyoke College. All had had elementary work in psychology and several had served before as subjects of psychological experiments. They did not know the purpose of the tests and were told not to discuss their preferences with one another.

The method of tabulating the judgments was this: If a subject said that in the series, I., II., III., IV., No. III. was most agreeable, then that figure was credited with three points, because it was preferred to the three other figures. The second choice was marked two points because it was preferred to two others. The third choice was marked one because it was preferred to one other, and the fourth figure was marked zero. When the tests were repeated the figures were marked a second time, and the two sets of marks added. In cases where the subject was unable to choose the count was divided between the figures. Thus if the subject said that I. and II. were the best, two but she could not choose between them, each of these figures was given two and one half points, because first choice

<sup>1</sup> *PSY. REV.*, 1906, Fechner number.

counted three and second choice two. Counting up the preferences of all subjects in this way it appeared that with the dark background the preferences were distributed as in Table I.

TABLE I

Color Combination	Fig. I	Fig. II	Fig. III	Fig. IV
Blue-yellow:				
No. of prefs.....	65.5	72	138	72.5
Percentage.....	18.8 +	20.6 +	39.6 +	20.8 +
Red-green:				
No. of prefs.....	67	91	106	84
Percentage.....	19.2 +	26.1 +	30.4 +	24.1 +
Blue-red:				
No. of prefs.....	72.5	77.5	118.5	79.5
Percentage.....	20.8 +	22.2 +	34.0 +	22.8 +
Green-yellow:				
No. of prefs.....	75	72	126	75
Percentage.....	21.5 +	20.6 +	36.2 +	21.5 +
Blue-green:				
No. of prefs.....	77.5	78	120.5	72
Percentage.....	22.2 +	22.4 +	34.6 +	20.6 +
Red-yellow:				
No. of prefs.....	58	86	137	67
Percentage.....	16.6 +	24.7 +	39.3 +	19.2 +

In every color combination, then, the same figure received the greatest number of preferences, namely, the one in which small masses of bright color in the center are surrounded by large masses of darker color in the periphery. Moreover the combination which shows greatest disparity of brightness, the blue-yellow, shows greatest excess of preference for Fig. III. while the combination which shows least disparity of brightness, the red-green, shows least excess of preference for Fig. III.

In order to see whether these results would be modified by the brightness of the background against which the colors were shown all the above tests were repeated, substituting for the dark background a light one made of Hering gray No. I. The whole designs were then framed in light gray frames. The results are as given in Table II.



TABLE II

Color Combination	Fig. I	Fig. II	Fig. III	Fig. IV
Blue-yellow:				
No. of prefs.....	40.5	100.5	132	75
Percentage.....	11.6+	28.8+	37.9+	21.5+
Red-green:				
No. of prefs.....	65.5	87.5	110	85
Percentages.....	18.8+	25.1+	31.6+	24.4+
Blue-red:				
No. of prefs.....	63	75.5	114	95.5
Percentage.....	18.1+	21.6+	32.7+	27.4+
Green-yellow:				
No. of prefs.....	62.5	91.5	123.5	70.5
Percentage.....	17.9+	26.2+	35.4+	20.2+
Blue-green:				
No. of prefs.....	57.5	86	126	78.5
Percentage.....	16.5+	24.7+	36.2+	22.5+
Red-yellow:				
No. of prefs.....	48.5	95	138	66.5
Percentage.....	13.9+	27.2+	39.6+	19.1+

Adding together the whole number of preferences for each figure irrespective of color combinations there are

	Fig. I	Fig. II	Fig. III	Fig. IV
With dark ground.....	415.5	476.5	746.0	450.0
With light ground.....	337.5	536.0	743.5	471.0

The most noticeable difference lies in the relative increase of preference for II. over I. on the light ground.

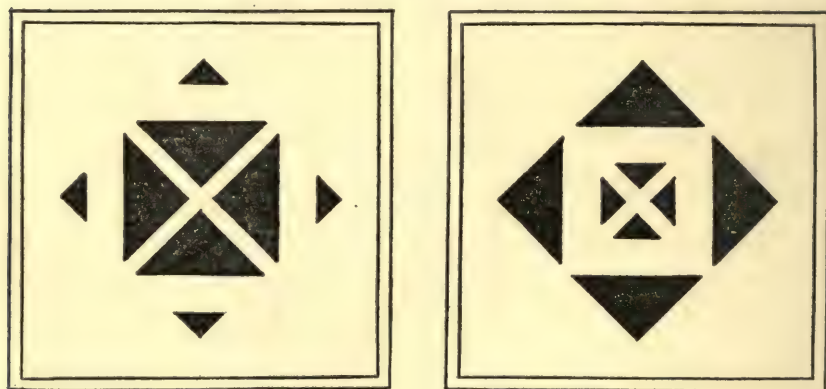
For the sake of isolating the several factors which contributed to the pleasantness of Fig. III. tests were next made to find out whether there was any preference for Figs. III. and IV. over I. and II. apart from the color combinations involved. The two figures were therefore shown side by side each filled out with a single color as in Fig. 3, I. and II. Here the color being constant the choice lay solely with the figure. It is conceivable, however, that the color chosen to fill them out with might affect the choice of the figure. Hence the two figures were filled out with each of the four colors in turn. These tests

were all repeated with the position of the figures reversed. Twenty-seven subjects took part.

TABLE III

Total No. of Prefs.	Fig. 3, I	Fig. 3, II
Filled out with blue.....	23	31
Filled out with yellow.....	20	34
Filled out with red.....	19	35
Filled out with green.....	21.5	32.5
Total of Prefs. on Light Ground	Fig. 3, I	Fig. 3, II
Filled out with blue.....	24	32
Filled out with yellow.....	21	35
Filled out with red.....	20.5	35.5
Filled out with green.....	26.5	29.5

There was therefore a constant preference for 3, II., as a figure, but, as Tables I. and II. show, this preference could be neutralized by other factors. The subjects who preferred 3, I.,



I.

II.

FIG. 3.

said that it seemed more free and graceful than 3, II. Those who preferred 3, II., said that this one seemed more compact and unified.

The next step in the analysis of Fig. 3, III., was to abstract from the relative size of the peripheral and central masses, and to see whether the bright color is preferred in the center apart from any effect of large and small masses. Two designs were made in which the central and peripheral masses were equal in

extent. Fig. 4, I., shows the lighter color inside and 4, II., the darker inside. The results were, for twenty-nine subjects:

TABLE IV

With Dark Ground	Fig. 4, I	Fig. 4, II
Blue-yellow.....	38.5	19.5
Red-green.....	23	35
Blue-red.....	36	22
Green-yellow.....	38	20
Blue-green.....	39.5	18.5
Red-yellow.....	33	25
With Light Ground (15 Subjects)	Fig. 4, I	Fig. 4, II
Blue-yellow.....	20.5	9.5
Red-green.....	19.5	10.5
Blue-red.....	20.5	9.5
Green-yellow.....	20	10
Blue-green.....	19.5	10.5
Red-yellow.....	20	10

It appears, then, that with one exception (the case of the red-green combination on the dark background) the majority of preferences falls to the figure with the brighter color in the center. The reversal in the case of the red-green may be better understood in view of the results to be reported in part II. below.

For the sake of trying what effect a contrasting frame might exercise on these choices a series of tests was made in which the colors were shown on the light ground but surrounded by the black frames. Fourteen subjects took part, and the tests were repeated in reverse order.

TABLE V

Light Ground with Dark Frames	Fig. 4, I	Fig. 4, II
Blue-yellow.....	14	14
Red-green.....	12	16
Blue-red.....	14	14
Green-yellow.....	12.5	15.5
Blue-green.....	9.5	18.5
Red-yellow.....	9.5	18.5

The results show a striking reversal of judgments for the most part, *i. e.*, in every combination the figures with the dark color in the center are liked as well or better than those with the bright centers. The explanation I believe to be this: the rim



of black around the edge of the whole design makes a rhythm with the central color when this is a dark one. Only one subject, however, seemed consciously to realize the presence of

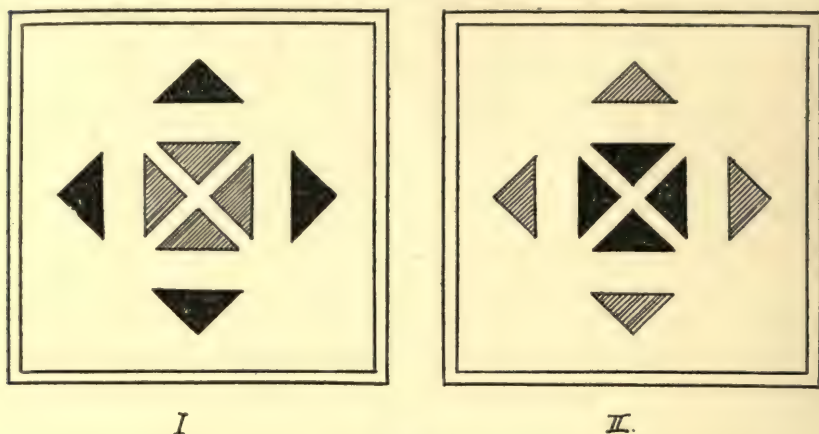


FIG. 4.

any such rhythms in the figures. No tests were made in which the dark ground was surrounded by a light frame. We must conclude that the brightness of the frame may have a very striking influence on the preferred color arrangement.

One further factor to be noticed is the preference for individual colors. One subject thought that she tended to prefer figures which had in them large masses of the favorite color irrespective of the arrangement. Each subject was asked to name the four colors used in order of their pleasantness (*A*) as seen on the dark ground, (*B*) as seen on the light ground. The color chosen first was counted three, the second two, etc.

## RESULTS FOR 28 SUBJECTS

Dark ground.....	Red	Yellow	Green	Blue
	54	49	38.5	26.5
Light ground.....	Blue	Red	Green	Yellow
	60	47.5	44.5	16

Thus the most agreeable impression was made by the blue on the light ground, and the most disagreeable by the yellow on the light ground. Adding together the total choices, however, we get Red, 101.5; Blue, 86.5; Green, 83; Yellow, 65. Yellow

called out the widest extremes of choice. By some it was very much disliked, to others it was slightly disagreeable or indifferent, whereas a few said it was by far the most agreeable. It appears from these tests that the tendency to choose large masses of a favorite color is not a dominant tendency; for on the dark ground red was the best color, yet, in combination, small masses of red are chosen with large masses of blue and of green.

## II

Seeing that the choice of colors as central seemed to be a function of their brightness, the next step was to equate the colors in brightness and to see what would then determine the choice. This part of the experiment was performed in a dark room. A light-proof box was made, inclosing an electric light. In front of the light was a ground glass plate which diffused the light over the designs which were fitted into the front of the box. The front of the box, which was 60 cm. wide and 30 cm. high, had grooves into which the cardboards carrying the designs in colored gelatines could be slid. The figures used were the same as Fig. 4, I. and II. above.

The colors used were the gelatine sheets furnished by the Stoelting Co. To match the colors in brightness two thicknesses of the blue gelatine were chosen as a starting point. The electric light was thrown through the glass plate and through the blue gelatines on to a screen at right angles to the light. At right angles to the screen a half-disc was set up which caught the direct light from a standard candle, the wick of which was kept as constant as possible. The candle could be moved until a point was reached at which, by the flicker method, the light from the candle was judged equal to the blue light. As a matter of fact the flicker never wholly disappeared with the blue light, but the minimum flicker, according to the middle judgment of three observers, occurred when the candle was 425 cm. distant from the half-disc. This, then, was taken as a standard, and the other colors were reduced to this brightness by the addition of sheets of gelatine. The judgments of two observers were taken for these equations and they coincided exactly. For the red, six sheets of gelatine

were necessary, for green, seven sheets, and for yellow ten sheets of gelatine and four sheets of yellow japanned paper. Of course the reduction of brightness by adding sheets of gelatine is not an ideally perfect system, because it does not give a continuous process, but it is only fair to say the the blending of the red, green and yellow with the candle light was very smooth and satisfactory, and if there was any objective difference in the brightness of the four colors it was very small, and, I think, negligible for the purpose of this experiment. The colors differed in saturation. Subjectively they all seemed pretty well saturated, but the spectroscope showed that, although red and green were good, the red lying between 640 and 620 on the spectrum scale, and the green between about 525 and 500, the blue transmitted violet, green and red, and the yellow transmitted green and red.

The experiment was conducted as above except that the subject sat in the dark until a signal was given and the electric light turned on which illuminated the colors from behind. As soon as the judgment of preference was given the light was turned off, and an interval of five minutes was allowed before the next pair of figures was presented. The subjects were all, with one exception, students in psychology at Teachers College. There were sixteen women and one man. The tests were repeated at a second sitting with the left and right position of the figures reversed. The results are shown in Table VI.

TABLE VI

Combination		No. of Prefs.
Blue-yellow:	Blue preferred as center.....	8
	Yellow preferred as center.....	26
Red-green:	Red preferred as center.....	24
	Green preferred as center.....	10
Blue-red:	Blue preferred as center.....	9
	Red preferred as center.....	25
Green-yellow:	Green preferred as center.....	12
	Yellow preferred as center.....	22
Blue-green:	Blue preferred as center.....	11
	Green preferred as center.....	23
Red-yellow:	Red preferred as center.....	23
	Yellow preferred as center.....	11



An examination of this table shows that in every color combination the color which is chosen as the center is the one which stands first in the order of the spectrum. Thus if we add together the total number of preferences for designs having red as a center we get 72, with yellow as a center 59, with green as a center 45, and with blue as a center 28.

I have no explanation to suggest for these preferences, but it seems probable that whatever factor was operative to determine choice in these last tests will serve to account for the case noted above in Table IV., where red was preferred over green as a center, though there the green was brighter.

#### SUMMARY

1. When large and small masses of color appear together, it is more agreeable to find the large ones in the periphery of the visual field.

2. Brighter colors are preferred near the center of such figures, darker colors near the periphery, whether the background of the colors is light or dark.

3. In figures where the central and peripheral masses are equal in size, and where a light background is surrounded by a black frame, a dark color is preferred in the center.

4. There is probably some tendency to prefer large masses of a favorite color, but this tendency does not prevail over other considerations.

5. When colors are equated in brightness the color which stands nearer the red end of the spectrum is preferred in the center.

# AN OPTICS-ROOM AND A METHOD OF STANDARDIZING ITS ILLUMINATION

BY C. E. FERREE AND GERTRUDE RAND

*Bryn Mawr College*

## I. INTRODUCTION

In a previous article<sup>1</sup> the statement was made by the writers that all comparative estimates of the sensitivity of the retina to color (limens or limits) should be made in daylight instead of in the dark-room. This is to eliminate the influence of the field surrounding the colored stimulus, and of the preëxposure. When the surrounding field is black, white is induced by contrast across the stimulus color. Since the colors all differ in brightness,<sup>2</sup> the induction takes place in different amounts for the different colors. This white, in proportion to its amount, reduces the action of the colors on the retina. Further, a given amount of white affects to different degrees the action of the different colors on the retina. To eliminate this two-fold unequal action, the surrounding field should be made in each case of the brightness of the color to be used. This can be done by working in a light-room of constant intensity of illumination and by making the surrounding field of a gray paper of the brightness of the stimulus color. In order to accomplish this and at the same time be able to work in any meridian of the retina we choose, we have constructed a special piece of apparatus which we call a rotary campimeter.<sup>3</sup> The influence of preëxposure is even more important than of sur-

<sup>1</sup> Ferree and Rand, 'A Note on the Determination of the Retina's Sensitivity to Colored Light in Terms of Radiometric Units,' *Amer. Journ. of Psychol.*, 1912, XXIII., p. 331.

<sup>2</sup> In a later paper, one of the writers (Rand) will show that it is of no advantage to equate in brightness in determining the limits of color sensitivity, and that harm results in so many ways from the attempt to equate, that it is doubtful whether it should be done even in determining the limens of color in the more sensitive parts of the retina.

<sup>3</sup> See C. E. Ferree, 'Description of a Rotary Campimeter,' *Amer. Journ. of Psychol.* 1912, XXIII., pp. 449-453.

rounding field. If the preëxposure is to black, white is added as after-image to the stimulus color. The effect of a black preëxposure upon the stimulus color is greater than the effect of a black surrounding field because more white is added as after-image of preëxposure than is induced by contrast from the surrounding field. This effect also can be eliminated only by working in a light-room of constant intensity of illumination and choosing as preëxposure a gray of the brightness of the color to be used.

Standardization for either one of these factors, however, can be accomplished for one degree of illumination only.<sup>1</sup> As the general illumination changes, the relation of the brightness of the preëxposure and of the surrounding field to the brightness of the colored stimulus changes. It is obvious, then, that if standardization is to be accomplished with regard to the influence of either of these factors, some means must be devised of maintaining the general illumination of the retina constant. No satisfactory method has as yet been obtained for keeping the illumination of a room by daylight constant. To keep it constant presupposes what has not as yet been provided, namely, a sensitive means of measurement. Constancy may be approximated by artificial illumination, but no artificial source of light has yet been devised which gives a light that approaches average daylight<sup>2</sup> sufficiently closely in composition to warrant its use in color work. Of the various sources of light the Moore Tube comes nearest to doing this, but spectrophotometric and colorimetric determinations show that the light from it contains an excess of blue,<sup>3</sup> and, therefore, although it

<sup>1</sup> When the colored light used to stimulate the retina is independent of the general illumination, *e. g.*, when it is obtained from the spectrum, from monochromatic sources, or from standard filters, these two factors alone will modify the result of the color observation. If, however, light reflected from a pigment surface be used as stimulus, a change in the illumination will in addition change the amount of colored light coming to the eye.

<sup>2</sup> For results of measurements of the color values of average daylight, see Nichols, E. L., *Transactions of the Illuminating Engineering Society*, 1908, III., p. 301. Ives, H. E., 'The Daylight Efficiency of Artificial Illuminants,' *The Illuminating Engineer*, 1909, IV., pp. 434-442; and 'Color Measurements of Illuminants,' *Transactions of the Illuminating Engineering Society*, 1910, V., pp. 189-207.

<sup>3</sup> See Ives, H. E., 'Color Measurements of Illuminants,' *Transactions of the Illuminating Engineering Society*, 1910, V., p. 206; and Rosa, E. B., quoted by Moore,



has been adopted by various textile concerns for use in color matching, its substitution for daylight can scarcely be recommended for the more exact requirements of color optics. Ives and Luckiesh<sup>1</sup> attack the problem of producing artificial daylight from another side. By their subtraction method they claim to have gotten the closest approximation to average daylight yet attained. They aim to cut out by absorbing screens the excess of red and yellow in artificial light due to the comparatively low temperature of artificial illuminants. Tungsten lamps are used by them as the source of light, and two kinds of commercial glass approximating in their absorptive action cobalt blue and signal green are used as screens. In order to correct for the pronounced band of yellow-green transmitted by the cobalt blue, a film of gelatine dyed with rozazeine is also used. Although according to comparative measurements made by Ives and Luckiesh the light thus gotten is the closest approximation to average daylight yet obtained, still it shows a deficiency of 15 per cent. in the green and about 25 per cent. in the blue. Moreover the spectrum of this light does not show the brightness distribution of the spectrum by daylight. Since the absorbing screens cut down the light emitted by the tungsten lamp to 15 per cent. of its original intensity, the spectrum of the light finally given out shows the brightness distribution characteristic of lights of low intensity. We seem thus compelled either to give up the investigation of the color sensitivity of the retina for daylight illumination, or to devise some means of keeping this illumination constant. At an early stage of our work of standardizing the factors extraneous to the source of light, we were compelled to take into account the influence of the changes in the illumination of the visual field upon the color observation. The changes of illumination that took place from day to day, the progressive changes during the day, and the many sudden changes even in the course of an hour, rendered any constancy, or close reproduction of results entirely out of the question.

D. McF., 'A Standard for Color Values,' *Transactions of the Illuminating Engineering Society*, 1910, IV., p. 224.

<sup>1</sup> Ives, H. E., and Luckiesh, M., 'Subtractive Production of Artificial Daylight,' *Electrical World*, 1911, LVII., pp. 1092-1094.

In order to obtain a standard illumination, two things are necessary: (a) A means of controlling the illumination must be provided, which is sufficiently sensitive to cause small changes. (b) A method of measuring the illumination produced has to be devised; at least, a means must be secured for determining when an illumination has been obtained that is equal to a given preceding illumination. It is the purpose of this paper to describe an optics-room provided with means of control which we have found adequate to meet the above requirements; and to state a method of identifying and reproducing any given illumination of this room.

## II. DESCRIPTION OF OPTICS-ROOM

The dimensions of the room are  $12\frac{1}{2} \times 10$  ft. It is situated on the upper floor of an isolated building and is lighted by a skylight  $8 \times 7\frac{1}{2}$  ft. Beneath the skylight two diffusion sashes,  $4 \times 7\frac{1}{2}$  ft., are swung on hinges so that they can be raised or lowered as desired. The framework of these sashes is made of light-weight iron. For convenience of local control of illumination, if needed, each sash is divided into four units by means of cross-pieces. The sashes are filled with double-strength glass ground on one side, so adjusted to the frame that they can be removed easily for cleaning or for the substitution of some other kind of glass in case that is desired. This glass diffuses the light so effectively that local shadows cast by the cross-pieces in the framework of the skylight are completely eliminated, while the sudden changes of illumination produced by the passage of the sun behind a cloud are reduced to a minimum. This diffusion seems to have the further advantage of reducing the yellowness of direct sunlight below the limen of sensation. At least, when working under the sash, the observer never judged a gray exposed through the campimeter opening as yellow under any local conditions, as frequently happened when working under direct sunlight.

The room is planned also so that small changes of illumination can be produced, ranging from the intensive illumination of a south-exposure skylight to the blackness of a moderately good dark-room. Two provisions are made for this. (1) The

diffusion-sashes are made so that any or all of the panes of ground glass can be quickly and easily taken from the sash, and anything can be substituted that is desired; or the illumination can be varied by placing layers of tissue paper above the glass. (2) The room is provided with two curtains mounted on heavy spring rollers. One is a white curtain made of thin muslin; the other is a black light-proof curtain so mounted that, when drawn, its edges are deeply enclosed in light-proof boxing extending along the four walls of the room. One or both of these curtains can be drawn any distance that is desired, and the illumination can thus be changed gradually from a very intensive brightness to a fairly good blackness. To aid in getting dark-room effects, the doors of the room are carefully boxed and curtained. One requirement of a perfect dark-room, however, is lacking, namely, the walls and floor of the room are painted white. This is because it is of advantage in the light-room work, and because complete blackness is not needed in the type of work for which the room is devised.

### III. METHOD OF STANDARDIZING

As stated earlier in our paper, no satisfactory means of determining the amount of daylight illumination in a room has been provided by the physicist, so there is little hope at this time of solving the problem from that side. The brightness induction of the peripheral retina, however, has been found by us to be extremely sensitive to changes in the general illumination. This phenomenon seems to provide us with a sensitive measure of these changes while, at the same time, it represents the combined effects for sensation of the principal subjective factors that might vary from day to day.<sup>1</sup> To apply the method in its most sensitive form, the inductive power of white was chosen because it is the most strongly affected by illumination changes. For example, when No. 14 Hering gray was used as stimulus and white as campimeter screen, a noticeable change was produced in the induction when the white curtain of the optics-room was pulled forward 1 cm.<sup>2</sup>

<sup>1</sup> This means of identifying the illumination of a room was devised by Rand.

<sup>2</sup> The sensitivity of this method of detecting changes in the general illumination was compared with the sensitivity of the Sharpe-Millar portable photometer. In



from a position in which its edge was directly above the long axis of the campimeter. This caused a change in the illumination of the room so small that it could not be directly sensed. Further, at 11 o'clock in the morning of a bright day in September, when a point at  $25^\circ$  on the nasal meridian was stimulated, one of the writers (Rand) reported that the white screen induced black across the stimulus No. 14 gray to an amount that caused it to equal in brightness  $107^\circ$  of black and  $253^\circ$  of No. 14 gray; at 2 o'clock of the same day the induction was increased until the No. 14 gray matched  $150^\circ$  of black and  $210^\circ$  of the gray; at 4 o'clock of the same day the No. 14 gray matched  $180^\circ$  of black and  $180^\circ$  of the gray.<sup>1</sup> Working at  $25^\circ$  in the temporal meridian, this observer reported at different times during one day and on different days, the wide variations shown by the following figures:  $283^\circ$  of black,  $225^\circ$ ,  $145^\circ$ ,  $190^\circ$ ,  $238^\circ$ , etc. Another observer (Miss Campbell) reported less induction, but her variations from time to time were equally great. At  $25^\circ$  in the temporal meridian, she found at different

this photometer one of the comparison fields is illuminated by the light of the room and the other by a standard tungsten lamp enclosed in the photometer box. When the room is illuminated by daylight, the field receiving the light of the room is seen as white while the field lighted by the tungsten lamp appears as a saturated orange. The difference in color between the two fields renders the photometric judgment difficult and makes the instrument very insensitive for daylight tests. For example, our tests showed that by the method for identifying an illumination described in the text, a change in illumination could be detected which was produced by drawing the white curtain 1 cm. from a position in which its edge was directly above the long axis of the campimeter. But with the receiving surface of the portable photometer in precisely the same position as the stimulus screen of the campimeter, the edge of the curtain had to be moved 11.3 cm. in order that the change of illumination might be detected. Moreover, this amount of change could be detected only in case the photometric field was continuously observed while the curtain was being drawn, in which case the comparison field was observed to be slightly darkened. The judgment was made, then, in terms of a just noticeably different brightness of the field which was illuminated by the daylight, rather than in terms of a disturbance in the brightness-equality of the two fields. When, on the other hand, the judgment was made in terms of a just noticeable disturbance in the equality of the two fields, as the judgment would have to be made if the photometer were to be employed for the reproduction of any former illumination taken as standard, the curtain had to be drawn 44.2 cm. before the change could be detected. This j. n. d. represents an amount of illumination equal to 2.5 foot-candles.

<sup>1</sup> This increase in the inductive action of the screen caused by the decrease in illumination, was accompanied by a shrinkage of the zones sensitive to color covering an area of  $4^\circ$  to  $6^\circ$ .

times  $80^\circ$  of black,  $103^\circ$ ,  $160^\circ$ ,  $175^\circ$ , etc. After a careful study of the phenomenon with different screens and with different backgrounds, the inductive action of the white screen upon a stimulus of No. 14 Hering gray, at  $25^\circ$  in the temporal meridian, was found to provide the best means of detecting changes in the illumination of the optics-room. At this point on the retina, the induction was by no means minimal, nor was it sufficiently great to cause the medium gray chosen for our stimulus to appear too dark to give a small j. n. d. of sensation. Having thus provided ourselves with a means of producing small changes of illumination and a method of detecting them, we had in order to complete our work but to choose an illumination for each observer, which could be used as standard. Since we wished to work on both light days and days of medium darkness, an average had to be chosen as our standard from the measurements obtained on a number of days ranging from light to dark, so that on bright days, the room could be darkened, and on dark days it could be lightened until this value was obtained. For observer *A* (Rand) an illumination was selected which caused an induction of black across no. 14 gray stimulus viewed at  $25^\circ$  in the temporal meridian to an amount which caused the gray stimulus to equal in brightness  $210^\circ$  of black and  $150^\circ$  of no. 14 gray; for observer *B* (Ferree)  $180^\circ$  of black and  $180^\circ$  of no. 14 gray; and for observer *C* (Campbell)  $145^\circ$  of black and  $215^\circ$  of no. 14 gray. The amount of black induction was identified in each case by means of a measuring-disc made up of sectors of black paper and no. 14 gray of the Hering series. This measuring-disc was carried by a motor and placed just behind the  $25^\circ$  point. The observer fixated the  $25^\circ$  point and compared the gray of the measuring-disc as seen in central vision with the gray of the stimulus seen  $25^\circ$  from the fovea. The sectors of the measuring-disc were then changed until the two grays were of equal brightness.

Previous to each series of observations the illumination of the room was changed until the amount of brightness induction was brought to the value chosen as standard. It was tested at intervals during the sitting and was readjusted when

necessary. Details of the method of doing this are as follows. When the white screen and the no. 14 gray stimulus had been put in place, the observer took his position and adjusted the fixation-knot in front of the motor for the  $25^{\circ}$  point on the temporal meridian. The measuring-disc set at the standard value was mounted on the motor. The observer reported whether the stimulus appeared lighter or darker than the measuring-disc, or of a brightness equal to it. If the judgment lighter or darker was given, the curtain was drawn one way, or the other until the stimulus accurately matched the measuring-disc in brightness.

This method not only gives a sensitive measure of the changes of illumination of the visual field and a successful means of standardizing the illumination of a room by daylight, but it has in addition advantages for work in psychological optics not possessed by an objective standardization, could that be successfully obtained. The problem of standardization includes more for the psychologist than it does for the physicist, for the former has variables to take into account in addition to the changes that may take place in the energy of the stimulus. Even though the illumination of the room be made objectively constant, we should expect variations in the response of the retina to this illumination because of its own changes from time to time. Brightness contrast, for example, might be expected to vary from sitting to sitting even when the stimulus conditions are kept absolutely constant. Two factors would be concerned in these variations: changes in the inducing power of the surrounding parts of the retina, and changes in the sensitivity of the local area. These changes would take place even when the usual precautions known to the experimenter in this field have been observed. Such precautions are commonly limited to fatigue, adaptation, etc. These precautions do not provide for the changes that occur in the retina from day to day. Moreover, they do not adequately guard against the variations to which they are intended to apply, for no precaution can adequately guard against a change in a factor, unless some measure of that factor be had. So far as the writers know, in these general



precautions intended to keep the state of the retina constant, no measure of the variable factor has been provided to test the adequacy of the method. The method proposed by us, however, is planned with this in view. It takes into account not only the objective, but the subjective variables, and reduces both to a constant. For example, when no. 14 gray surrounded by the white field is made equal to the measuring-disc composed of  $210^\circ$  of black and  $150^\circ$  of the no. 14 gray for observer *A*, it means that the observation may be begun with the assurance that the total result of all the factors—the illumination of the room, the local sensitivity of the retina, and the inductive action of the surrounding parts of the retina—is the same as in the preceding observation.

What has just been said should not be considered as more than a general statement of the application of the principles of the method. In actual practice a greater refinement of working may be attained. If, for example, one wishes to use a preëxposure differing in brightness from that of the colored stimulus, and doubts whether a test which covers only the local sensitivity of the retina and the inductive action of the surrounding parts is a sufficient check upon the after-image sensitivity, he may make his standard include the effect of the preëxposure he wishes to use. In short, if he does not consider adequate the more general test we have described, he may duplicate, in establishing his standard, any combination of brightness factors, due to preëxposure, brightness of screen, or what not, that he may wish to use in his experiment proper.

The test of a method is how well it works. The test of this method is that we shall be able closely to duplicate our results from sitting to sitting regardless of the changes in the outside illumination from day to day or from morning until afternoon. The method stands the test. Long series of observations in the peripheral retina show a very small M.V.—much less even than is shown in the ordinary color observations in the central retina, where, as compared with the peripheral retina, the factors extraneous to the stimulus exert little influence.

The following table has been compiled from a number of observations to show the variations in the results of color limens and color limits (*a*) when the general illumination was controlled according to the method described above, and (*b*) when no more precautions were observed than were used by previous investigators. In previous investigations of the color sensitivity of the peripheral retina, care has been taken to work only at the same hours of days that appeared equally bright, or, if on days of different brightness, to make a rough approximation of preceding illumination by means of curtains without using either a definite standard or means of measuring. For our work with the illumination controlled, the gray of the brightness of the color at the illumination selected as standard was used for the preëxposure and the campimeter screen. For the work without especial control of the illumination, the gray of the brightness of the color for one of the days selected as typical was used throughout for preëxposure and screen. This gave in the first case complete elimination of the effect of preëxposure and surrounding field, and in the second case elimination as complete as could be gotten without accurate control of the general illumination. Results are given in the table for blue and green only because the sensitivity to these colors is affected most by changes of illumination.

Stimulus	Illumination	Screen and Preëxposure	Variations of Limits on Different Days	Variation of Limens on Different Days
Green.....	Controlled.....	Gray No. 9	0°	0° <sup>1</sup>
	Uncontrolled.....	Gray No. 8	4°-6°	60°-82°
Blue.....	Controlled.....	Gray No. 33	0°	2°-3° <sup>2</sup>
	Uncontrolled.....	Gray No. 32	4°-5°	18°-30°

<sup>1</sup> The limen for green was taken in both cases at 25° on the temporal retina.

<sup>2</sup> The limen for blue was taken in both cases at 40° on the temporal retina.

## THE SENSATION OF MOVEMENT

BY JOHN E. WINTER

*University of Michigan*

A number of psychologists and physiologists have undertaken the study of the location of the sensation of movement in its various phases with varying results. Among those who have reached different conclusions, in greater or less degree are Nagel, Strümpell, Goldscheider, Angier and Pillsbury. Goldscheider<sup>1</sup> reached his conclusions after experimenting along three different lines: (1) Moving the finger by means of weights, (2) passing a current through the joints, and (3) rousing reflexes by means of tapping on the joint, applying acids, etc. (1) He experimented on the index finger by means of weights. The hand was placed in a static position with only the end of the finger free. A cord fastened at one end to the tip of the finger was passed over a pulley and a delicately poised scale was attached to the other end. By putting weights on the scales the finger could be moved until the subject detected the sensation. A needle attached to the scales recorded on a revolving drum the results of the experiment. The pressure of the string on the finger naturally produced a skin sensation, but Goldscheider asserts that in addition to this sensation another sensation was also recognized, easily distinguishable from the skin sensation, and located apparently in the joints. (2) To substantiate this result further experiments were made with an electric current passed through the fingers at different places, particularly the joints, with the result that when the electrodes were placed over the joints sensitivity was in every case reduced greatly. (3) He then experimented on the reflexes in frogs and rabbits. He attempted to produce a reflex in frogs by stimulating the surface of the hip and shoulder

<sup>1</sup>GOLDSCHIEDER. Ueber den Muskelsinn und die Theorie der Ataxie. *Gesammelte Abhandlungen*, 9-96. Ueber die Empfindlichkeit der Gelenkenden. *Ibid.*, 282-287



joints. An ordinary contact stimulus gave no reflex, while a light tapping on the joint produced movement, although Goldscheider himself admitted that in tapping other factors might have entered in to produce movement. Acids which did affect the adjacent skin had no effect whatever on the joint. With the rabbit the breathing reflex was used as an index of sensitivity but no uniform results were obtained. The skin of the leg was cut open to lay bare the bone and experiments were made on the periosteum, capsule and marrow. A light stimulus applied to the joint produced no reaction and when a stronger stimulus was used it sometimes resulted in reactions and sometimes not. Thus it might be inferred that sensation can be produced by mechanical means in the joint but it is not thereby proven that the joint is the seat of sensation. Reflexes were also produced by stimulating the periosteum and the marrow.

Goldscheider came to the conclusion that sensations of movement arise (1) from the rubbing of the articular surfaces and the wrinkling of the capsule, (2) strain on the tendons of one set of muscles and relaxation of their opposites, (3) change in the form of muscles. This opinion was very generally accepted until the first conclusion was criticized by Pillsbury<sup>1</sup> who proved first, that by passing a current through the wrist there followed nearly as marked a decrease in sensibility as when the current was passed through the elbow itself; and second, that the sensory innervation of the joint has not been definitely proven by histologists.

Angier<sup>2</sup> experimented with the lower arm and confined its movements to a horizontal direction. His problem was to determine the accuracy of judgment in comparing the distances of points by movements of the elbow. The details of his work are irrelevant to our discussion but some of his inferences are of interest. Angier believes with Goldscheider that sensation of movement comes largely from the joints. The condition of the muscle, whether normal, flexed or extended did

<sup>1</sup>PILLSBURY. Does the Sensation of Movement Originate in the Joint? *Am. Journ. of Psych.*, 1901, 12, 346-353.

<sup>2</sup>ANGIER. Die Schätzung von Bewegungsgrößen bei Vorderarmbewegungen. *Zeitschr. f. Psychol. u. Physiol. d. Sinnesorg.*, 1905, 39, 429-448.

not materially affect his results, nor did the position of the arm within certain limits play an important rôle. In my experiments, however, I found that the angle of the arm and its position with reference to the body profoundly influenced results.

Strümpell<sup>1</sup> is inclined to ascribe the chief sensation of movement to the muscles, but he gives no reason for his opinion. He proved, however, that muscles could stand great lesions without disturbing the accuracy of perception. This would seem to combat his own theory, though it is not necessarily a disproof.

Nagel's<sup>2</sup> opinion is that sensations of movement cannot be derived from the skin. As Pillsbury believes that sensations of movement come from the tendons and muscles, the conflict seems to be chiefly between the rival theories, the joint theory and the muscle and tendon theory.

In Pillsbury's experiments little attention was paid to introspection but conclusions were drawn from an interpretation of physical results supplemented by an appeal to well-established physiological data as to the location of sensory endings. In our experiment there was also a minimum of introspection, the subject being merely asked occasionally whether he felt movement in any particular place. In one set of experiments, however, no heed was given to limina and the subject was asked to concentrate his whole attention in an endeavor to locate the place of sensation.

The following investigation was conducted at the suggestion of Professor W. B. Pillsbury, to whom as also to Dr. J. F. Shepard, the writer is indebted for suggestions and criticisms.

The subjects were Professor Pillsbury (*P*), Dr. Shepard (*S*), Messrs. Work (*W*), Woodrow (*Wo*), Cook, Osborn, and the writer (*Wi*), all of whom had had previous psychological training. Experiments were confined to measurements of the limen of movement of the elbow, normal and with current through the upper and lower arm, elbow, wrist and hand.

<sup>1</sup>STRÜMPELL. Ueber die Störungen der Bewegungen bei fast vollständiger Anästhesie. *Deutsche Zeits. f. Neuroh.*, 1903, 23, 1-38.

<sup>2</sup>NAGEL. Die Lage-, Bewegungs- und Widerstandsempfindungen. *Handbuch des Physiologie des Menschen*, III., 733-806.

The apparatus was the same as that used by Professor Pillsbury in his experiments in 1900. Since Spearman<sup>1</sup> criticized Pillsbury on the ground that the speed was too slow, four speeds were used. A hinged board served to support the arm which was raised by a cord passing over a pulley in the top of an upright. The cord was attached to one of a series of pulleys which were connected with a worm gear and run by means of an electric motor. A pointer in the end of the board passed over a millimeter scale on the upright and served to measure the amount of movement of the arm.<sup>2</sup> In all the experiments the subject sat with his arm resting upon the board, the elbow near the hinge. The angle of the elbow varied in the successive experiments from 75 to 150 degrees.

As indicated in the tables the first series of experiments were taken with the arm normal; then a series was taken with an induction current running through the elbow, wrist, tips of fingers, palm of hand or muscles and tendons of the lower arm. A series was also taken using ether for anæsthesia in place of an electric current. The apparatus was arranged so that the downward movement could be measured as well as the upward movement. The figures in the tables represent the averages of experiments ranging in number from 10 to 210.

When all was in readiness the motor was started and the operator gave a signal about two seconds before the board began to rise. The signal was given at this time in order that the subject might concentrate his attention to catch the first sensation of movement. A few experiments were taken without signal to test the subject's error of anticipation.

Of the four speeds used the first was the slowest, the second a little faster and the fourth was the fastest of all. A comparison of these speeds in terms of seconds is given below (Table IV).

A study of the tables given herewith will reveal among other things the following:

1. *The Influence of a Particular Angle.*—In Table I. the

<sup>1</sup> SPEARMAN. Fortschritt auf dem Gebiet der Psychophysik der räumlichen Vorstellungen. Tastsinn. *Arch. f. d. ges. Psych.*, 1906, 8, 1-51.

<sup>2</sup> The length of the arm board from hinge to pointer was 52.4 cm.



angles for each respective set of experiments were uniform and hence allow of no generalizations. In Table II. *Wo's* results in general, both normal and with current, in speeds 1 and 2, going up, show a limen for angle 135 to 150 degrees about twice as large as the limen for angles 75 to 80 degrees. In speed 4, this judgment is reversed, and in speed 3 the results obtained on different days are so various that other factors, physical or mental, must have entered. The results going down vary apparently without any relation to the size of the angle.

*Wi's* results in speed 2, going up, also show a larger limen for the larger angles.

TABLE I

	Elbow Angle		Speed		Normal		Cur. to Elbow		Cur. to Muscle		Cur. to Wrist	
	Up	Down			Up	Down	Up	Down	Up	Down	Up	Down
W.	113	91	1	Tests	210 mm.	20 mm.	50 mm.	20 mm.	40 mm.	20 mm.	50 mm.	20 mm.
					4.7	3.0	7.2	4.0	17.5	7.4	7.1	4.9
					5.2	3.5	6.0	3.2	16.8	5.0	6.0	3.0
					5.5	3.5	7.1	3.9	19.0	6.8	7.6	5.5
	113	91	2	Tests	4.6	3.7	5.8	5.0	11.8	6.0	5.9	4.6
					60	30	20	10	20	10	20	10
					4.9	6.9	5.3	5.6	14.3	6.1	13.3	6.9
					5.4	6.1	7.3	5.7	13.0	4.8	10.0	8.6
P.	123	51	3	Tests	6.8	4.6	9.0	4.3	15.3	5.6	12.2	7.6
					4.8	4.9	13.5	6.5	9.2	4.0	9.6	14.7
	123	51	4	Tests	20	10	10	10	10		10	
					2.8	5.9	8.9	9.8	6.1		4.5	
					4.6	4.0	6.8	9.5	6.0		5.3	
					5.6	4.7	8.1	9.2	7.5		6.0	
	135	117	1	Tests	5.7	4.8	7.1	8.3	5.6		3.9	
S.	135	117	2	Tests								
	135	117	3	Tests								

Speeds 1 and 3 partly reverse this order, but a new factor enters here,—the position of the arm with reference to the body. With the lower arm parallel to the line of the chest, angles 75 to 80 degrees gave a smaller limen than angles 135 to 150 degrees; but with the arm at right angles to the chest and with the elbow close to the body, the sensitivity with the small angles was greatly reduced. This was undoubtedly due to the extra pressure of the muscles against each other at the elbow. Inasmuch as the joint is little affected by the position of the arm in these experiments and the muscles are greatly

affected and the sensitivity reduced, the results would seem to corroborate Professor Pillsbury's theory of muscular sensation.

TABLE II

	Tests	Elbow Angle		Speed	Normal		Cur. to Elbow		Cur. to Muscle		Cur. to Wrist	
		Up	Down		Up mm.	Down mm.	Up mm.	Down mm.	Up mm.	Down mm.	Up mm.	Down mm.
Wo.	15	150	135	1	12.5	8.3	6.7	7.6	12.3	8.3	11.2	4.0
	18	75	75	2	5.3	6.1	10.3	4.0	16.8	6.2	10.5	6.0
	15	135	135	1	13.0	2.5	15.3	3.6	11.2	4.4	11.2	6.0
	18	180	80	2	6.3	4.5	8.1	3.0	7.7	4.9	7.5	7.2
	18	130	130	3	6.3	9.0	5.3	9.9	6.7	9.9	7.5	4.8
	18	130	130	3	15.6	5.2	23.1	15.7	28.	17.	30.5	5.4
	17	115	140	4	4.6	4.1	5.4	8.1	6.4	7.9	5.8	7.3
	18	130	130	4	3.1	5.4	2.5	5.7	4.4	2.7	3.1	4.0
	18	130	130	4	3.8	2.2	4.1	2.6	3.6	2.8	2.4	4.5
Wi.	15	130	130	1	6.9	2.6	16.4	2.5	28.6	11.6	30.6	9.4
	18	60	60	2	15.4	2.6	27.5	4.5	15.9	7.5	19.8	6.5
	15	130	150	1	17.9	6.3	35.1	10.8	30.0	16.8	29.7	13.4
	18	80	80	2	8.5	3.2	35.1	16.2	15.0	13.5	28.2	9.6
	15	150	130	3	6.3	4.6	12.8	10.5	22.8	17.1	22.2	15.0
	18	88	88	3	25.2	8.8	44.1	15.8	55.2	7.9	43.2	18.3
	18	85	85	3	18.1	4.2	43.7	9.6	48.6	6.9	51.7	7.5
	15	150	150	4	8.6	8.8	11.2	10.8	17.8	20.4	18.3	15.8
	18	175	176	4	9.1	7.1	19.3	9.3	24.2	6.2	25.8	9.4
18	85	85	4	8.9	3.0	22.2	7.4	16.4	8.3	39.0	10.4	

2. *Comparison of Ups and Downs.*—*Wo's* results for speeds 1 and 2, going down, have predominantly the smaller limen. This is true of all of *W's* results, of *P's* speeds 2 and 3 and my speeds 1 and 2. With the other speeds the results either vacillated or were reversed. *S's* results show that for normals the up movements had the larger limina and when the current was applied the down movements had the larger. Thus there seems to be little uniformity and the results warrant no generalization.

3. *Factors Influencing Judgment.*—(a) Accuracy of judgment is greatly influenced by the physical condition of the subject, and also by the peculiar position of the body. With the head leaning forward on the hand the limen immediately increased, as it did also when the head was allowed to hang forward. Remarkable differences in results, using the same angle and same position but on different days, are seen in *Wo's* figures for speed 3, the limen ranging from approximate equality to a ratio of 4 to 1.

(b) The time of giving the signal. This is an important factor. Suggestion plays a strong rôle. The subject soon gets into the habit of expecting to feel movement at a certain time after the operator says 'now' and this causes large errors of anticipation. If the signal is given at a stipulated period of time, known to the subject, it is impossible to tell exactly just how many judgments are real judgments of movement. The number of zeros or mistaken judgments revealed by the records by no means tells the whole story. Often the subject was sure of movement sensation when the board was perfectly still. Occasionally the unevenness of the electric current was responsible for errors. Anæsthesia by means of ether obviates this difficulty to some extent. But if the subject was deceived once when he was positive he felt motion, why is it not possible that every other judgment of movement was only the result of imagination? This applies particularly to speed I which is so slow that even when the subject knows his arm is moving he frequently is unable to detect the sensation.

Again, in speed I we experimented to ascertain whether the sensation remained constant after movement had been (apparently) felt, and we found that sensation was not continuous but would be lost to consciousness at intervals and after a few seconds reappear. This indicated, then, a fluctuation either of attention or of the sensation of movement. This phenomenon offers an interesting problem to be worked out, although its presence detracts largely from the reliability of the results as a basis for scientific inferences regarding the location of kinæsthetic sensations.

Giving the signal about two seconds before the arm rises affords a uniform and constant expectation and the error of anticipation is perhaps reduced to a minimum. Varying the interval between signal and rise of board from one to ten seconds increases the error of anticipation. Giving the signal just as the board begins to rise vitiates the experiment as it scatters the attention for the moment, during which time movement might have been felt. The same is true if the signal is given shortly after the board begins to rise. Experiments show that under these conditions the limen rose considerably. The



constant gradual motion had already begun before the subject fixated his attention and there was no period of change from no-motion to motion in his attention.

(c) Degree of attention is also an important factor. Occasionally when the subject had been giving several judgments whose limen was found to be much larger than ordinary, he would 'buckle down' and the limen would immediately decrease.

(d) Elimination of the signal. A number of experiments were taken without giving a signal,—the operator at first following the old method of allowing the board to rise or fall at stated intervals, and later holding the board from 1 to 90 seconds. The results are here given.

TABLE III

	Tests	Elbow Angle		Normal		Cur. to Elbow		Cur. to Muscle		Cur. to Wrist	
		Up mm.	Down mm.	Up mm.	Down mm.	Up mm.	Down mm.	Up mm.	Down mm.	Up mm.	Down mm.
Wo. . .	15	145	80	5.5	8.0	6.2	4.8	6.5	4.0	9.9	3.2
Wi. . .	18	160	85	21.1	9.0	26.7	16.0	26.8	15.7	50.8	19.0

Naturally the long waits increased greatly the error of anticipation, and many erroneous judgments were made. The figures which show only the averages really give a deceptive idea of the experiment. In reality the limen varied from zero judgments to 88 millimeters. All of my results without signal were much larger than those with signal, but *Wo's* limen without signal was as small as the smallest of the others. He seemed all along to be possessed of very keen sensitivity. My results show that several times, after waiting as long as from 50 to 90 seconds, I yet made a wrong judgment.

4. *Comparison of the Speeds.*—Spearman (6) criticized the low speed used in Professor Pillsbury's experiments. To obviate this difficulty we used four speeds whose rates were as follows:

TABLE IV

Distance traversed, 0 mm. to 10 mm.

Speed. . . . .	1	2	3	4
Seconds. . . . .	13.34	5.24	4.30	1.90

Speed 1, it will be seen, is very slow, the board moving only ten millimeters in thirteen seconds, while speed 4 is rapid. The results obtained with speed 1 were the least satisfactory, especially when a current was used. The subject would frequently say, "I think I feel movement but am not sure," and even when the subject knew his arm was moving he frequently could not detect sensation.

5. *Location of the Seat of Sensation.*—Anæsthetizing one part of the arm is sure to result in a partial anæsthesia of the neighboring parts by a kind of irradiation. This complicates the problem, as we cannot tell just how large a share of the effect we are to attribute to the anæsthesia of the particular part in question. Acting on the general principle, however, that the part to which the current is applied will be most affected by it, we may say that to establish Professor Pillsbury's theory from experimental results it will be necessary to show that when the current is applied to the muscle, or the union of muscle and tendon, the sensitivity is reduced as much as when applied to the joints. *W*'s records show in practically every case that when a current was applied to the muscle of the arm the sensitivity was less than with the current applied to either the wrist or elbow. *P*'s record, going up, generally coincides with *W*'s. His records going down show no uniformity. In *S*'s case, the sensitivity was most reduced when the current was passed through the elbow, and in *W**o*'s case and my own there is absolutely no uniformity.

A comparison of the limina with current applied to the wrist and elbow, in succession, fully substantiates the contention of Professor Pillsbury that anæsthesia of the wrist causes almost as great a decrease in sensitivity as anæsthesia of the elbow. In several cases the limina for the wrist were greater than those for the elbow. A series of experiments were taken also with a current applied to the upper arm, and it is significant that the limen thus obtained was about twice as large as the limen for the normal experiments.

It has seemed to the writer that introspection during the experiment should play a larger rôle, and that its results would be more conclusive than the objective study of limina. In our

experiments the subject was sometimes asked where he felt sensation, and the answer showed that most of the sensations were located in the finger tips and hand, while scarcely any sensations were felt in the joint or muscle. *Wo* insisted throughout that his sensations were nothing but skin sensations, and in the experiments conducted as those described above I incline to the same view. With the up movements there was a sensation of 'pushing up' from beneath, and with the down movements there was a kind of 'sinking' feeling throughout the hand. With regard to this sensation it may be well to note that if they are in reality skin sensations they are entirely irrelevant to the problem and should be ignored; for it is certain that they would not be felt if the arm were raised without the aid of the board underneath. If, on the other hand, these sensations are more deeply seated, though seemingly located in the skin, the probability is that they are muscle and tendon sensations stimulated by the contraction of the muscles and tendons at the elbow with which they are intimately connected.

To obtain a little more introspection, a series of experiments was taken in which the subject was asked to attend, not to the moment *when* sensation was felt, but *where* it was felt. Almost every conceivable answer was given, showing conclusively that mere introspection could never solve the question as to the location of sensation. Here are a few of the answers given: skin and muscle; under wrist; elbow; finger tip; wrist to finger; muscle at elbow; whole arm; arm and shoulder; muscle under arm; near wrist; skin at elbow, etc., etc.

The last series of experiments were the most interesting and, I believe, the most fruitful of results. It was found in the preceding experiments that running an electric current through the finger tips and the ball of the thumb did not result in perfect anæsthesia of these parts. The sensation was still located there more than anywhere else. To make sure of the anæsthesia of these parts, as well as the skin under the muscle, ether was applied on sponges with the result that no sensations were felt there. The introspective results are noteworthy. The subject announced that he no longer had a skin sensation



of any kind but that a different sensation was now felt (different in kind) and located very perceptibly in the muscle and tendon. It would seem that these results come nearer a solution of the problem than the former ones. Ether produces an unnatural state of the organs, to be sure, but not so unnatural a state as the electric current produces. The current produces motion through the whole arm and increases the difficulty by compelling the subject to discriminate between movement caused by the rising board and that caused by the current. The element of false movement is at least eliminated with the use of ether, thus giving a better opportunity for introspection.

Before giving a summary of our results it might be well to mention the statement of Dr. Shepard that most of his laboratory students in the last ten years, several hundred in all, have obtained results similar to those given here.

If we bring together the results of the experiments, it seems that the effect of passing a current through the elbow is not as Goldscheider assumes to anæsthetize the surface of the joints but to anæsthetize the muscles and possibly the tendons about the joint. That this is true is seen from the fact that a similar current passed through the muscles anywhere else has the same effect, and in much the same degree. The current passed through the upper arm, the forearm, or the wrist tends to increase the limen and in many cases more than when passed through the elbow. This conclusion is confirmed by introspection. The sensation of movement is almost without exception assigned to the fingers, muscles of the forearm and other muscles or tendons or the surface of the body over them. It might be noticed in this connection that the illusion of pushing up at the elbow when mercury is poured from a beaker or when a heavy weight is lowered upon a cushion is always referred to the muscles below the elbow, not to the joint, and is to be explained from the sensations in the muscles themselves, not to the pressure that they exert upon the joint surfaces. In fact there is no single bit of real evidence that the sensations of movement come from the joint surface. This experimental result is in harmony with the statements of the histologists that there are no sense endings

on the joint surfaces. The sensation may come from the capsule, from the ligaments and from the muscles and tendons that are involved in the movements but not from the joint surfaces.

#### SUMMARY

1. Passing a current through the wrist reduces sensitivity as much as passing the current through the elbow. This would seem to refute the evidence offered by Goldscheider for proving the joint to be the seat of movement sensation.

2. An apparently better way to indulge in introspection is to use ether to anæsthetize the skin.

3. The results obtained with the use of ether tend to corroborate Professor Pillsbury's theory that the muscles and tendons are the seat of the sensation of movement.

## MIND AS MIDDLE TERM

BY ROBERT MACDOUGALL

*New York University*

Into much recent discussion there enters, in some aspect or other, the controversy as to what place the mental system shall be accorded in psychological science. The problem reappears in many guises and general statements as well as particular working conceptions have been formulated regarding it. The preliminary chapters of text-books customarily define the author's position in the matter, and in numerous papers and addresses of late special modifications of the psychological conception of mind and reconstructions of the limiting criteria by which the field of investigation is determined have been made.

The older psychology was not troubled by doubt in regard to these matters. It defined psychology in terms which gave a precise formal limitation to its field; and since, supported by philosophical and theological assumptions, the distribution of its subject-matter was restricted to mankind and approached by a purely introspective method, the maintenance of a clear demarcation of its province from that of adjacent sciences presented no great difficulty. The relation of consciousness to physical structures and changes was incidental not essential. The soul in its temporary alliance made use of the body as an instrument, but in action as well as quality was ultimately independent of the latter. It was not considered in reference to either a determining stimulus or a necessary reaction. In itself the mind composed a unity of functions and the object in studying it was to determine the place of each of these functions in a rational system. Psychology was thus concerned with the logical problem of the mind's constitution and its direction in ideal activity.

In this conception of mind as a self-contained system of phenomena whose limits are stated strictly in terms of consciousness psychology has concurred in its more recent defini-



tions as well. The formulations with which current text-books introduce their subject-matter adhere to this postulate. Psychology is the science of self and the facts of self as manifested in individual experience; it is concerned with psychical phenomena, conscious processes or psychoses; the description and explanation of the phenomena of mind or consciousness is its aim. In these mental activities and conscious states as such psychological interest centers. Its business is the systematic exploration, under the methods which inductive science imposes, of the constitution of mind and its internal correlations.

This substitutes an empirical study—systematically directed introspection under experimental control—for the logical reflection upon which earlier rational psychology depended. Nevertheless its field is defined in similar terms, as the system of psychical activities or phenomena. The psycho-physical correlations which may exist, whether conditioning or dependent, are incidental to the discussion. The study of stimulus and reaction may be helpful in many ways to psychological science, but with neither of these, if we adhere to the implications of such current definitions, is the latter directly concerned. It is the reaction in consciousness which follows external stimulation or physiological change in the one case, and in the other the mental complex which, generically or in particular, precedes a given form of reaction that alone affords material for psychological study.

But in the more recent development of the science this conception has undergone a variety of modifications due in part to natural changes accompanying the extension from within of the field of psychological investigation, and in part caused by external pressure through the study, from the standpoint of independent sciences, of those phenomena with which the correlations of mind bring it into contact. The movement from within has been a complex one. Its most obvious constituent is to be found in the development of comparative psychology and its extension both of the experimental method of study and of the guiding conceptions of psychology until by a succession of rapid strides the whole animal kingdom from man to protozoan had been included within its field of research.

The enormous multiplication of individual types to be studied, in which this extension has resulted, is accompanied by a still more profoundly modifying factor, namely the extreme qualitative variety of the forms of consciousness to which in the course of his work the psychologist must adapt his conceptions. The simple and precise formulæ which served the earlier human and classic psychology fall to pieces when the psychic life of microorganisms is to be included along with the complex and highly articulated consciousness of man in a common system. This problem has been met, by the psychologist himself, in a variety of ways. To one phase he has responded by simplifying and universalizing the essential constituents of the unity of consciousness, as when the existence of irritability, discriminative selection and adaptive reaction, demonstrated in the simplest organic types, are construed as a manifestation of the psychological trinity—affection, cognition and volition. To another aspect of the problem he has reacted by substituting for this unitary system of common characters the conception of individual mental functions, such as sensitivity, organic memory, space-orientation, learning by imitation, and the like, the evolutionary history of each being traced as the succession of organic forms is passed in review. In a similar way the comparative psychologist has modified his working conceptions to meet still other demands imposed by the continuous extension of his field.

A second general constituent in this modifying process is to be found in the complication of phenomena by which the psychological student is confronted within the limits of human experience itself, or of the human type. In this field extension has taken three general directions. The first is from the normal through the exceptional, abnormal and pathological to the final disintegration of the unitary self in individual impulses, elementary idea-systems and persistent reaction-types. This field has hitherto been the most productive of such supplementary modifications. It has occasioned the conceptions of subliminal or subconscious phenomena, of psychic disaggregation and split-off selves, of motor and psychic automatisms, with a host of other working hypotheses.

The other extensions lead, the one downward through sub-normal and defective types, imbeciles and idiots, towards a limit which the anencephalous monster may be taken to represent; and the other backward from adulthood to youth and infancy, and from foetal to embryonic conditions until, in the fertilized ovum, it meets the comparative psychologist on common ground in dealing with a simple undifferentiated organism. The need of devising, in the service of continuity, an adequate system of conceptions thus receives a new and greater emphasis.

All these demands have arisen from within in the course of the psychologist's own work; but to the readjustments by which they have been met he has been urged by an independent and extraneous stimulus. This also has had a complex character, covering both systems of physical change with which mental activity is correlated, the field of the stimulus and that of the reaction.

The study of the physical conditions of consciousness, especially of its physiological locus, has been approached in independence of any primary interest in mental phenomena. While the psychologist has availed himself largely of the results both of physical and physiological research in the technical arrangement of his problems as well as in the correlation of results, he has not been hampered with any confusion, as to aims or methods, between the general province of physics and that of his own special studies. The science of optics, for example, is not confounded with the study of space perception nor climatology with psychic reaction to weather changes. In the case of physiology, however, the uniformity of association between the primary series of reactions in nervous and other tissues and the mental activities with which the psychologist deals has led to the inclusion of the latter group of facts within the system of phenomena which, in the most general sense, is to be considered. For physiology these mental reactions can never become an independent system coördinate with the neural processes, and the account it gives of them assumes their dependence upon physiological changes throughout. Thus the psychosis is conceived not as a psychological object but merely



as one product of nervous action, and its treatment constitutes only a highly specialized topic within physiology at large.

The study of sensory stimulation in all its forms and of the mechanism of simple and complex reaction-types has been so extensive in recent years and the mass of detailed information it has afforded concerning psycho-physiological correlation has been of such importance to psychology that physiological methods and conceptions have attained dominance in this field even with psychologists. A sketch of the nervous system and its functions is made the introduction to psychological study; mental processes are explained in terms of nervous habits and rearrangements; in general, a treatment of the direct psychological relation of experience to the external world as condition and object of the will's reactions is replaced by speculative constructions of its physiological relation to mediating processes in the central nervous system. These modifications in the psychologist's working conception often involve more than a reformulation of criteria and amount to a plain confusion between the standpoints of independent sciences.

The second of these two external influences proceeds from the biological study of reactions. The biologist not infrequently makes use of the physiological method in his work, and a rigid application of his own postulates would perhaps require the application of this conception throughout; but, as distinguished from the latter, ecological biology is concerned not with the special mechanisms of reaction but with their teleological relations. It construes the response of the organism in terms of its serviceableness to some end, and its object is to determine the complex of adaptations which thus characterizes the systematic reactions of any given type or individual. Each organism maintains certain permanent relations with the environment. Its energies are directed to securing food, shelter, warmth, protection and alliance—in a word, provision for the satisfaction of certain needs and desires. Each reaction may therefore be conceived in terms of its approximation to the realization of those conditions which determine the maintenance of these relations in an ideal form. Many such adaptive reactions we know to be pervaded by

consciousness; to a much greater range we impute the same general character to them, and it may perhaps be questioned whether the terms in which the biologist defines his object have, in strictness, any meaning apart from the implications of consciousness and its values. But at least the biologist does not start from this assumption. Reaction consciously directed to an end is but a special form of response having its place in a more general field of organic adaptation with which, as a whole, the science is concerned. Biology, in this phase of its work, is the science of behavior, whether behavior be construed in terms of consciousness or not.

In recent years, as the systematic study of life in its ecological relations has extended, it has been brought into more intimate as well as more extensive contact with the system of phenomena which functional psychology considers in its study of mental adaptation. Especially is this true in the case of comparative psychology where method is necessarily objective. These two sciences may be said to have come face to face in the study of animal behavior. The results in this field have been similar to those already pointed out. The difficulty of securing a satisfactory criterion for determining the distribution of consciousness, especially in view of the apparent variability in its association with a given function, and the sense of identity in the fundamental nature of behavior in all organic species has led to modifications not merely in the formulæ applied to particular types of life but to a recasting of the terms in which the subject-matter itself is stated—for example, when the scope of psychology is defined as a study of ‘organosis’ in its most general application.

In this new approximation towards a neighboring science the primary conception from which procedure starts is again objective, but instead of the conditioning stimulus it is now the consequent reaction which becomes the determining element. Behavior must always be considered, it need not be said, as well as physiological function and external stimulus. The mind is historically and socially conditioned in reaction as well as incitement, and its materials of expression must be regarded equally with its provoking stimuli. This modifica-

tion, however, goes farther than to employ the form of response as a means of interpreting the subjective attitude. It proposes a study of the objective rearrangement in its effects upon the conditions of life as a substitute for the inquiry into the forms of conscious activity by which, under certain limitations, such readjustments are characterized.

In still other connections the same general question as to the nature of the system of facts with which psychology is actually concerned has been raised. One of these may be used to point the consideration which all in common provoke, since it is not dependent upon the complex extensions of recent psychological investigation into fields which bring it into contact with the physical and biological sciences but has been introduced as a comment upon the earlier classic method of introspection by the normal human mind. It is the query concerning what is actually meant by the terms mind and experience, mental facts and mental laws, as psychology conceives them.

In this particular modification of the central psychological conception mind is construed as the system of characteristics and habits which the individual presents in his reaction to stimuli. It is what the mind does, not what it is, which is here considered; and what it does is expressed in its attitudes and social reactions. What comes under review by the psychologist, according to this conception, is not the form and conditions of the mental activity as such but its logical and practical aspects, its products and consequences. These lie open in some degree to even the casual observer, and the intimate companion of any man is in a position to make a comprehensive system of judgments concerning the character of his mind in this sense of the term. It is thus we learn the habits and character of the individual, the range and accuracy of his knowledge; by observing the plans he has formed and carried out or given up we judge his originating and organizing capacity and his tenacity of purpose; similarly, we may know his predilections in a multitude of affairs and be familiar with his general tastes and desires.

But it may be questioned whether in the existence or



acquisition of such knowledge the essential attitude of the psychologist is at all embodied, for it rests primarily upon the determination of relations between two objective series, that of the stimulus and that of the reaction; and leaves in doubt the place of the intermediate system of mental activities from which the psychologist takes his departure. Now there are two distinct points of view from which mind or experience may be conceived; the one regards the qualitative, the other the relational aspect. The first of these standpoints is sometimes called the subjective, the second the objective point of regard.

Under the first conception immediate, irreducible experience is intended, the fact, namely, that existence has at each moment a unique qualitative character which constitutes it a moment in the concrete history of an individual subject. In its raw immediacy one thus knows yellow, noise, cold and pain; one feels satiety, longing and dislike. With this character a second subject cannot be brought face to face, nor can it be shared with him. The experience of another simply is not, and cannot become, my experience; no adequacy of constructive interpretation, no sense of sympathetic intimacy, no accordance in social reaction will annul this fact. My mind is not obscured from the view of other minds, when I conceive mind in this way, because of the complexity of its workings or the deviousness of its course, but because—to continue the figure—it is not at all a visible object. It is hidden because it is inaccessible. To know another mind, in this sense of the word, is to be that other; that is to say, it is to deny the fact of otherness and to bring the event in question within the category of immediate experience. Whatever the status of this qualitative aspect of existence in reference to any specific problem, and whether it concerns the psychologist's work or not, the uniqueness and exclusiveness of subjective immediacy in each individual experience is a fact to be recognized, not a theory to be discussed.

From the second standpoint mind is treated in terms of its relations to the objective world. Whatever the qualitative aspect of any experience it springs from certain stimuli and results in specific reactions. It is in these physical and social

connections that the observer is interested. His point of departure is in a system of reality lying beyond the experience of the moment and his return is to that larger world again. The mind, in this case, is but the point where a stimulus has effect and a reaction is originated. To know it means to be acquainted with the characteristic response which is made by it to any situation. When this response is conceived in terms of the physical reactions necessary to the maintenance of life and of the social adaptations of which our fellow-men can take account, it is not essential that the qualitative nature of the experience as it exists for the subject should be taken into account. In the reckoning which the observer makes the mental system may be ignored, for it is the characteristic reactions to which it leads in their objective and social forms alone in which he is interested. So long as his knowledge of the sequential connections between typical stimuli and the responses which the individual makes to them is secured the immediate quality of experience which mediates between the two series is negligible.

Such knowledge may even be more exact and complete when the observer is a bystander than when he is himself the subject of the experience. The occupancy of the locus of experience in no way ensures an acquaintance with the real character of the mind in this sense of the term. One's estimate of his own capacities may be farther astray than that of the impartial onlooker, and his reaction in any given case may surprise him as really as his acquaintance. It is indeed the latter to whom we look for a sound judgment in regard to such a matter, for he is undisturbed by that emotional excitement which is inseparable from personal experience and unoccupied with attention to the purely subjective aspect of the situation from which the experient can never wholly free himself.

If we regard the mind from this objective standpoint it is obviously neither inaccessible nor hidden. One's character is recognized as widely as acquaintance extends. To one person it is known less fully than to another; to one this series of reactions is more familiar than that, and thus individual estimates of character vary; but to all alike the data for such knowledge

are accessible, the subject himself having simply the position of one observer among an indifferent many.

But this way of dealing with reactions is obviously defective, whether it be considered practically or theoretically, since knowledge of the situation involved is incomplete in regard to stimulus and reaction alike. For the stimulus is not the object as it exists for the onlooking individual or is objectively defined; it is the object as presented in the experience of the subject himself. The reaction, similarly, is not the gross physical movement or socially discernible adaptation; it is the whole attitude of the self aroused by the situation which is thus presented. A full description of the stimulus in physical terms is indeed conceivable, but this cannot rest upon any analysis of the constitution of the external object alone; it must include the organic reaction which this stimulus provokes and thus be finally stated in physiological terms. The reaction, likewise, if its full description in physical terms is attempted, must be conceived as the whole complex readjustment, peripheral and central, which the physiological stimulus has evoked. That such a multiplicity of elements of physical change exists, occurring in the body at large and constituting a physiological analogue to both the complex mental situation which is presented and to the reaction in consciousness which it occasions, is a methodological assumption and not a field of data accessible throughout its range and utilizable by the observer in making up his account. This holds true also in regard to practical affairs, for the onlooker is constantly driven to recognize the insufficiency of his knowledge of the real stimulus to which response is made, and the incompleteness of his acquaintance with the reaction itself.

Thus even when the observer's interest lies wholly outside the limits of individual experience the subject's report of any situation is indispensable to the completion of his data. If he had possession of all the facts regarding either the physiological effects of the stimulus or the final readjustment within the body which it arouses he might be able to predict the reaction upon the physical or social environment, but such knowledge is inaccessible and the only alternative is to find how the situation



presented itself to the subject in question and what his real and complete response to it was, whether such response resulted in the immediate production of changes observable by the on-looker or not.

The psychologist's standpoint cannot be identified with either of the points of view above described. Each individual experience possesses a subjective quality which is at once unsharable and indescribable. It bears also certain relations to both antecedents and consequents in the external world which it is practically important that the subject and his fellow-men alike should understand. But the psychologist is occupied neither in demonstrating the qualitative uniqueness of each individual experience nor in tracing its practical consequences in the form of movements. His standpoint is subjective but not qualitative; it is relational but not objective. The first of the two points of view above described is subjective and qualitative, the second is relational and objective, while that of the psychologist is subjective and relational. His study is of facts which cannot be objectively discerned, though their existence may under certain conditions be inferred from objective data, but the facts thus revealed through intuition he treats in terms of their relations, in whatever direction these relations lead him.

In general the plexus of connections in which any individual experience stands may be treated in terms of a three-fold grouping. The first of these is the relation of antecedence, in which the experience is studied in connection with its conditions, whether these lie within the course of previous experience or derive from the external world in the form of so-called stimuli. The second is the relation of reciprocity, including the material study of the constituents of each individual experience and the formal study of resemblances and correlations among the phases of such experience. The third and last is the relation of consequence, in which the influence of the event upon both the course of subsequent experience and the forms of expression by which mental activity is characterized are studied.

The second of these three groups is, by definition, restricted

to the immediate phenomena of subjective experience. It conforms most closely to the conceptions and methods of the earlier introspective psychology. In contemporary science the observation of it is systematically controlled and extended statistically. In comparison with the traditional conception of mental constitution its scheme has consequently undergone both complication and reconstruction; but in general data and products this part of psychology remains essentially unmodified. It is an analysis of the psychic system itself to determine its constituents and the forms of their combination in the various orders of synthesis by which mind is characterized. Concerning this part of the psychologist's work, therefore, disagreement is not likely to arise.

It is in the first and third of the foregoing sections, in which the two-fold correlation of consciousness with its physical environment is treated, that the danger exists of obscuring if not obliterating the fundamental conception which both defines the subject-matter and determines the limits of psychology. In these fields of study, quite as much as in connection with that central system of facts to which normal introspection has been directed from the beginning, consciousness with whatever that term implies must remain the final point of reference if psychology is to have any independent existence. Otherwise its province will simply be parted between physiology which invades its field from the side of the stimulus and biology which encroaches from that of the reaction. For psychology these correlations of consciousness are necessarily secondary and contingent. To assume either correlative as a dominant conception, that is, to define the province of investigation in terms of the stimulus-field or of the reaction-system, carries one beyond the circle of psychic phenomena into the world of physical materials and their changes, and to combine the two as is the tendency in much contemporary writing makes of psychology a pseudo-science created merely by taking slices from two independent sciences and combining them. The maintenance of psychology rests upon a clear definition of its aim as a science and a perception that the system of consciousness presents a substantial and unitary subject-matter which

cannot be dissipated in a confused treatment of individual topics in physics, anatomy, physiology, biology and anthropology.

If, however, just this solution of continuity is to be avoided the centrality of consciousness must never be lost sight of. This thesis has two points of application; the first touches the substantial existence of the mental system as the primary field of psychology, and the second concerns its primacy as an interpretative criterion in the treatment of its physical correlations. In the first place, then, the objective point of view is not homologous with the standpoint of psychology. Practical interest seeks only adaptation, whether it deal with things or men. It is concerned not with the mechanism of any change in itself but with its products or effects, and when it has to deal with minds it treats them in accordance with this general aim, classifying all reactions in terms of their relations to stimuli which constitute their nearest antecedents in the discernible series of changes which the objective world presents.

If now we can say that a given stimulus inevitably arouses the mental reaction in question and from such a mental reaction these physical consequences and no others proceed, the middle term which is thus repeated may be dropped from the series and the first and last terms connected directly. Towards this general conclusion all the conceptions above described tend. The field of actual transformations is reduced to the two physical systems and their contact is marked only by a theoretical division. But in the system of reality mind is not a mere point where stimulus and reaction meet, as these various modifications imply when carried to their logical conclusion. It is psychologically an interposed system in which the stimulus-field terminates and the reaction-system has its origin; and it is the existence of this mediating system which constitutes both the ground and limitation of the science. This interposition, as already indicated, implies no interruption of continuity; it is not a metaphysical solution but a methodological subdivision of reality which it involves.

Consciousness, in other words, does not possess an independent field which can be contrasted with that of stimulus



and reaction loci. It not only has a physical correlation in the physiological system of activities but may even be described as the flooding of these two fields with ideal values and direction. Nevertheless it both constitutes in itself a definite and complex system of phenomena and affords the only means by which an approach can be made to the problem of stating in its fulness the nature of either stimulus or reaction. This system, therefore, instead of receding to the vanishing-point becomes for psychology the central field of exploration within which repeated and extended analysis, by indirect as well as by direct means, reveals an endlessly increasing complexity and integration.

The second application of this thesis is in the psychologist's treatment of the physical changes with which consciousness is correlated whether as antecedent or consequent. It may be stated by saying that the conception of consciousness and its implications affords the determining reference which both defines the field to which the psychologist limits his activity and supplies the qualitative criterion which guides his work. The systematic reference of consciousness, as already indicated, is to the stimulus-field on the one hand and to the reaction-system on the other, and it has no third and independent theater of activity. But in the psychological treatment of these two groups of phenomena consciousness must remain a determining conception. The stimulus enters within the circle of consideration only when it ceases to be regarded in terms of physical change and is treated as the antecedent of a specific qualitative consciousness. The reaction, likewise, becomes subject-matter for psychology only when it is no longer conceived as a movement or material reconstruction but is construed as the embodiment of a particular mental attitude.

In both of these cases the situation, as psychologically conceived, is made to turn upon the presence of consciousnesses as its cardinal point. The stimulus is that which provokes mental activity, the response that which expresses it. Thus it assumes as its foundation the existence of affective sensibility and conative tendencies, of hedonic values and preferential reaction in the organism which thus responds to stimulation. To con-

sider irritability in the physiological meaning of the term alone, or reaction as organosis in the general sense of an adaptation which is not based upon consciousness is to relinquish this constitutive assumption and to make the implication of consciousness an incident in a larger system. But for the psychologist the elimination of consciousness in this way is simply pouring out the baby with the bath, and in all valid extension of his science or its underlying assumptions it will be found on closer inspection that this conception still functions.

The psychologist does indeed study the whole system of specific stimuli, whether physical or physiological, which acts upon the senses, as well as the characteristic reactions which the organism makes to them; but these are never, as a matter of fact, construed by him in thoroughgoing mechanical terms. It is the reactions of living creatures in which he is interested and the stimuli he considers are those only to which such organisms are irritable. Every force may logically be called a stimulus or reagent and every substance upon which it impinges may be said to present a reaction in the rearrangement of its physical relations which follows the collision. But no one advocates a modification in our conception of mental science which will make it coextensive with this whole field. Such an extension appears only in the metaphysical universalization of concepts, with which psychology as a special science has nothing to do.

Even within the field of organic life itself a division obtains between activities which are conceived to fall within the limits of psychological phenomena and those which are excluded from consideration. The former are not restricted to reactions dependent upon the coördination of many individual muscle groups, which can be construed only as reflecting the unity of the organism as a whole, but include also certain adaptive responses mediated by single organs and directed to the readjustment of the relations of that organ individually. Reflex action may be taken to represent this group. This conception, however, is not extended to the whole range of changes occurring within the organism. Absorption, osmosis and the chemical syntheses of nutrition are excluded from the

circle of phenomena which psychology treats. If the question be raised why these forms of reaction, together with such activities as capillary attraction and the selection and rearrangement of materials in the growing crystal, are thus excluded from consideration the distribution will be seen to turn upon the fact that in the case of these physical and metabolic reconstructions it has been found possible to treat the phenomena in purely mechanical terms as movements and combinations depending upon physical forces alone.

In its most extended form psychological treatment is thus still restricted to the sentient world. It is consciousness, in its most elementary forms indeed, but still consciousness, which determines where the line shall be drawn and not mere readjustment in the relations which characterize the system of physical materials. If any of the forms of change enumerated above, osmosis, absorption, etc., are included by any particular scientist it will be found that for him these processes are either steeped in consciousness at the moment of their occurrence, as for example, the activity of the organic cell at large has been conceived, or they are regarded as permanent forms of reaction the development of which has been mediated by consciousness in the past.

The system of habits represents this problem generically. The psychological student finds it necessary to contrast habitual reaction with the selective activity of consciousness. The formation of habit is marked by the progressive decline in directive attention. In its more established types it has already passed beyond the field of choice and control, while its theoretical limit is a complete dissociation from conscious activity, a condition which is at least approximated in the so-called vital functions, digestion, circulation and the like. The automatisms of habit therefore present in the highest degree the phenomena whose treatment these psychological extensions have been designed to serve. They are highly specialized adaptations in which the response to stimulation is direct and simple, depending upon no interposed activity of consciousness. As organic reactions they have teleological significance but they are independent of a psychical correlate.



If the field of psychology is to be redefined in terms of biological adaptation instead of mental process habit-automatisms seem to constitute at once the immediate occasion and sufficient justification for the change.

Nevertheless habit, which is tenaciously retained within the sphere of psychological discussion, maintains its place simply in virtue of the necessary relation to the selective and organizing activities of consciousness which is predicated of it. Though any given reaction of this type be now dissociated from consciousness and bring about by purely mechanical processes a teleological readjustment to changes in the external world it still logically falls to the psychologist to discuss it if it be thus construed as a product of consciousness, namely as a permanent reaction-type developed through the selective and organizing reactions of an antecedent mental activity. In this construction of habit, in which contemporary psychology at large agrees, the centrality of consciousness is maintained, for the habit-form is viewed in the light of psychic values and direction.

The application of this conception in the progressive extension of the field of psychological data is by no means restricted to the immediate reactions of the organism. It determines that growing system of investigations into culture and social history which depends upon the interpretation of permanent products of human activity, such as the monuments of literary and plastic art, or the industrial inventions and general material transformations which have been brought about in the service of mankind. These are legitimate fields of psychological inquiry because—and only because—of the implications of consciousness by which their treatment is everywhere suffused.

Wherever this underlying principle is applied the psychological point of view is assumed and the phenomenon becomes a datum for the science. It must therefore be said, I believe, that those investigations in which organic stimulation and reaction have been studied by other than physico-chemical methods involve the implication of consciousness, whether carried on by physiologists and biologists or by psy-

chologists themselves, and must in consequence be classed as psychological inquiries in the strict sense of the term. In some cases doubtless the application of this principle is due to confusion, but in others the scientist is under no illusion as to the nature of his work. Psychological science was first laid under obligation to physiology in this field, and that the debt is great as well as obvious a mere list of its contributors sufficiently indicates. More recently the tide has turned in the direction of biology whose students now hold the same general relation to experimental psychology which physiology possessed a generation ago. It may be assumed that an equal enrichment of the science is to be expected from this side also, and one which will react to the advancement of biology and its conceptions as physiological psychology has influenced the study of physiology. But in this general extension of knowledge it would be the very irony of fate if psychology were to lose sight of those distinctive conceptions upon which her existence as a science rests through a failure to apprehend the fact that all this constitutes primarily an enrichment of her own system of data, and that without such a fundamental reference to the forms and values of consciousness it can have no logical existence.

## DISCUSSION

### THE CASE AGAINST INTROSPECTION

It is rather generally agreed among English psychologists that there is something (state, process, act, relation, or whatever) which may properly be called *introspection*. There is also rather general agreement in the definition of the term, whatever may be said of divergences in regard to its practical application. The greatest disagreements have been over the temporal nature, the difficulty, and the reliability of 'introspection.'

It is now high time that we should question, more seriously than has been done heretofore, the existence of 'introspection' in the traditional sense. It is for this purpose necessary to present some actual usages of the terms 'introspection' and 'consciousness' in English psychology, although it is not at all necessary to go over the whole field of psychological writings and cull every instance in which use has been made of these terms. The discussion of the uses of *Selbstbeobachtung*, *Bewusstheit*, and other German psychological terms, is an entirely different piece of work which may or may not be profitable; it certainly is not profitable in English and I have no intention of engaging therein.

'Introspection' is usually defined in terms which are equivalent to the expression *consciousness scrutinizing itself*. Such definitions are significant only when 'consciousness' and 'scrutiny' and 'itself' or whatever terms are substituted for them are more explicitly defined. Typical statements from psychological texts are given below.

James says: "It means of course the looking into our own minds and reporting what we there discover. Every one agrees that we there discover states of consciousness" ('Principles,' I., 185). Angell: "It consists simply in the direct examination of one's own mental processes" ('Psychology,' 4th ed., 5). Judd: "In observing this conscious state, he introspects." Stout: "To introspect is to attend to the workings of one's own mind" ('Manual,' Introd., Ch. 2, 2). Stratton: "This direct acquaintance with the state of our minds which all of us to some extent possess" ('Experimental Psychology,' 2). Yerkes, in discussing 'introspection': "It is by observing my own consciousness that I directly study the objects of consciousness"



(Introduction, 41). Maher, the exponent of Thomism: "States of consciousness can only be observed by introspection—that is, by the turning of the mind in on itself" ('Psychology,' 4th ed., 11).

The technical use of the word 'introspection' in this way is of recent introduction (see Oxford Dictionary). But the signification is very old. We need not pursue it back farther than Reid, Hamilton, Bain and James Mill, to get a definite understanding of the extent to which 'self-consciousness' is involved in British theories. The discussion here runs into the consideration of the term consciousness, to which we must give a little space.

Bain<sup>1</sup> distinguishes and lists 13 different senses in which the term was used. The catalogue is now too short, for James's usage of the term does not belong anywhere in it. With the greater number of the uses we have no great concern. We should point out, however, that Reid made of consciousness a separate faculty, practically the 'introspective' observation of the modern psychologists (First Essay, Chapter 1). Hamilton while having some agreement with Reid in the use of the term, contended that consciousness is involved in every mental act: "Can I know without *knowing* that I know? Can I desire without *knowing* that I desire? Can I feel without *knowing* that I feel? This is impossible. Now this . . . common condition of self-knowledge, is precisely what is denominated consciousness" ('Metaphysics,' Lect. IX., p. 110, in American ed. of 1880. The whole of this lecture is especially important).

What we now call 'introspection' is described by Hamilton as follows: "In an act of knowledge, my attention may be principally attracted either to the object known, or to myself as the subject knowing: and in the latter case, although no new element be added to the act, the condition involved in it—*I know that I know* becomes the primary and prominent matter of consideration" (Lecture XI., p. 135).

In strong contrast with the use of the term 'consciousness' by Reid and Hamilton, we find James Mill declaring: "To say I feel a sensation is merely to say that I feel a feeling, which is an impropriety of speech. And to say that I am conscious of a feeling is merely to say that I feel it. . . . In the very word feeling all that is implied in the word consciousness is involved ('Analysis,' Ch. V.). To which Bain felt constrained to add a footnote correcting what he considered a serious error.

The modern views of 'introspective' consciousness are best represented by the statements of Stout and James, because these

<sup>1</sup> 'The Feelings and the Will,' 4th ed., 538, *et seq.*

two have made the attempt to work out a system in which 'introspection' is not only admitted, but is really provided for. I shall confine my discussion therefore to these two authors. Other introspectionists have simply claimed that 'introspection' occurs without trying to show the nature or details of the process.<sup>1</sup>

In Stout's writings there is less confusion between consciousness (in the cognitive aspect, at least) and the objects of consciousness, than in the writings of other psychologists. "Psychical states as such become objects only when we attend to them in an introspective way. Otherwise they are not themselves objects, but only constituents of the process by which objects are recognized" ('Manual,' p. 124). "The object itself can never be identified with the present modification of the individual's consciousness by which it is cognized. This holds true even when we are thinking about modifications of our own consciousness. The conscious experience in which we think of another conscious experience is always at least partially distinct from the conscious experience of which we think" (pp. 58-59). If we confine our discussion for the present to the realm of sensational consciousness, we find that the objects which the sensation cognizes are 'sensible qualities' (p. 57) or 'sensory elements' (p. 120).

The 'sensible quality' red, and the sensation of red, one would think, differ in that the redness is in the quality or is the quality; the sensation should have no redness, for it is an element in the process of perceiving red. This is apparently what Stout means, so far as the sensation is primarily concerned. But the sensation has the property of becoming secondarily an object for another psychical state, and then, of course, it has objective qualifications. Obviously the only quality which we can consistently ascribe to the sensation of red in its secondary capacity is the 'sensible quality' it cognizes in its primary capacity: "If we compare the color *red* as a quality of a material object with the color *red* as a quality of the corresponding sensation, we find the redness as immediately perceived is an attribute common to both. The difference lies in the different relations into which it enters in the two cases" (p. 123). (See also footnote, page 58.) The sensation, as an object has intensity, as well as quality (p. 30), and when referred to the physical world, is correlated with wave-length, and not with any 'sensible quality.'

Here we have the whole scheme of 'introspective' consciousness.

<sup>1</sup>This of course does not apply to those who explicitly hold to the scholastic doctrine of introspection. I hope to show in a later paper that in the scholastic doctrine of the intellect there is a good foundation for the doctrine of introspection.

A sensation, as such, is not an object, but the awareness of an object; hence it is not observable, but an observation. This Stout sees clearly, and grants freely, and so far we can go with him. But, demanding that the sensation shall be nevertheless observed (for what reason we shall see later), Stout assumes that the sensation which primarily is consciousness, or awareness, is, or may be, secondarily what it is not primarily, namely, an object for another awareness, which may be either subsequent to the first awareness or simultaneous with it (pp. 18-19).

We wonder indeed what the 'mind' is which 'one' attends to ('Manual,' Introd., 2) and we might indeed wonder what the 'one' who attends is: these apparently simple assumptions become exceedingly complicated and shaky when introspection is included. Surely the mind is not the mere sum of the processes, for we are told that "the most important drawback is that the mind, in watching its own workings must necessarily have its attention divided between two objects," implying that it is only one process after all which cognizes both objects; for that there should be any difficulty in one process cognizing one object and another process cognizing another object, whether the second object is or is not the first process, does not seem reasonable. Without question, Stout is bringing in here illicitly the concept of a single observer, and his introspection does not provide for the observation of this observer; for the process observed and the observer are distinct.

James's doctrine of 'introspection,' as stated in the *Principles*, is less inconsistent than Stout's. That James seriously doubted the actual existence of the machinery he built up in theory does not in any way lessen the need for its examination, since the influence of James's speculations concerning consciousness is unfortunately very strongly felt in psychology.

"There are realities, and there are 'states of mind,' and the latter know the former; and it is just as wonderful for a state of mind to be a 'sensation' and know a simple pain, as it is to be a thought and know a system of related things" (II., 5-6).

"The *relation of knowing* is the most mysterious thing in the world. . . . Knowledge becomes for him (the psychologist) an ultimate relation that must be admitted, whether it be explained or not" (I., 216).

Here is an unmistakable deviation from Stout. For Stout, the term 'mental process' applies to the *knowledge*; for James it is primarily the *knower*, and knowledge is assumed as an additional



process, with which James concerns himself little, although involving it freely in his system. "The passing Thought then seems to be the Thinker" (I., 342). This "thinker" knows external objects, and it also knows past thought.

"It may feel its own immediate existence—we have all along admitted the possibility of this, hard as it is by direct introspection to ascertain the fact—but nothing can be known *about* it until it is dead and gone" (I., 341).

'Introspection' is then for James, first, the knowing of the knower (not of the knowing), and secondly is always retrospection. The division of attention in regard to which Stout trips, comes in here however more legitimately. "The Thought, which whilst it knows another Thought and the Object of that Other, appropriates the Other and the Object which the Other appropriated" (I., 340) is manifestly doing double duty; is simultaneously observing two different things at once.

James and Stout agree in postulating an 'introspection' which makes objective that which is primarily non-objective, but differ in that while James is postulating the objectification of the subject, and not dealing at all with the *knowing*, although specifically postulating it in addition to the subject, Stout is postulating the objectification of the *knowing* and deals with a subject only illicitly.

The objectification of the subject is for James not an occasional matter, but an essential aspect of the functioning of the 'stream of consciousness.' "*The knowledge of some other part of the stream, past or future, near or remote, is always mixed in with our knowledge of the present thing*" (I., 606) although "A mind which has become conscious of its own cognitive function plays 'the psychologist' upon itself. It not only knows the things which appear before it; it knows that it knows them" (I., 272-3).<sup>1</sup> This psychologizing is apparently only a special development of the universal function of mind by which it preserves its unity through the present subject knowing or 'appropriating to itself' the past subjects.

The doctrine of the essentially retrospective nature of 'introspection' is very useful to James in defending the 'transitive' states of consciousness which he admits cannot be discovered by 'introspection.' "*For a state of mind to survive in memory, it must have endured for a certain length of time.* In other words, it must be what we have called a substantive state. Propositional and con-

<sup>1</sup> I must confess that in the above quotations I find more 'mixed in with the knowledge' than James explains, especially in connection with the knowledge of the future, but I think the general meaning is clear.

junctional states of mind are not remembered as independent facts — we cannot recall just how we felt when we said 'how' or 'notwithstanding.' Our consciousness of these transitive states is shut up to their own moment — hence one difficulty in introspective psychologizing.

Any state of mind which is shut up to its own moment, and fails to become an object for succeeding states of mind, is as if it belonged to another stream of thought" (I., 643-644).

The essential points in James's scheme of consciousness are *subject*, *object*, and a *knowing* of the object by the subject. The difference between James's scheme and other schemes involving the same terms is that James considers subject and object to be the same thing, but at different times. In order to satisfy this requirement James supposes a realm of existence which he at first called "states of consciousness" or "thoughts," and later, "pure experience," the latter term including both the "thoughts" and the "knowing." This scheme, with all its magnificent artificiality, James held on to until the end, simply dropping the term consciousness<sup>1</sup> and the dualism between the thought and an external reality.

'Introspection' can hardly be bolstered up by James's mechanical psychology. To assume that the thought of a cabbage knows a feeling of regret, and that the thought of a cabbage may in another moment be known in turn by the thought of a red necktie, is ingenious but ineffectual. As the knower in the act of knowing is not known, but is known only after it has finished its cognizing, the assertion that what is now known was once a knower remains a mere assertion to the end. All that James's system really amounts to is the acknowledgment that a succession of things are known, and that they are known by something. This is all any one can claim, except for the fact that the things are known together, and that the knower for the different items is one and the same. This further implication James does not escape, in spite of the assumption of a series of different thoughts assuming the knowing function, for after all, the knowing function is the same in each case; the thoughts all take the same point of view in knowing other thoughts or things and it is the point of view which constitutes the real I or subject.

The real claim to admission which 'introspection' holds in James's original scheme is therefore not based on the turning of a subject into an object, but on the existence of two sorts of objects. There are, according to James's 'Principles,' thoughts, which are known; and

<sup>1</sup> James, 'Does Consciousness Exist?' *Jour. of Philos.*, etc., I., 478; also, 'A World of Pure Experience,' *Jour. of Philos.*, etc., I., 538-541.

the things corresponding to the thoughts, which are also known. A cabbage is known, and there is also in the stream of consciousness a 'thought' of a cabbage, which is known, no matter by what. If this sort of representationalism is accepted, there is no objection to calling the knowing of the thought 'introspection' meaning therefore by the term exactly what Reid meant by 'consciousness.' But the day for such psychical mechanics has gone by. The ghostly world of representational 'ideas' or 'states of consciousness,' dim shadows through which we may look at the real objects casting them, or on which alone we may fasten our gaze, attracts no longer faith nor interest. It is significant in this connection that James, in giving up the term 'consciousness,' abandoned his whole representational scheme, without however giving up the essential mechanics of his doctrine of knowledge. Hence, for his last psychology, there is virtually no 'introspection' possible.

There are probably no psychologists at the present time who hold to 'introspection' explicitly on the representational grounds of Reid and the older view of James. If there are any such, I certainly do not wish to argue the point with them. For one who believes in representationalism a belief in representationalistic 'introspection' is quite the consistent thing.

I am inclined to suppose that the greater number of those modern writers who explicitly presuppose 'introspection,' have in mind, however dimly, the sort of 'introspection' which Stout defines.<sup>1</sup>

The objections to Stout's theory are not of the same order as the objections to the theory of James, although just as profound. There can be no denial of the existence of the thing (knowing) which is alleged to be known or observed in this sort of 'introspection.' The allegation that the knowing is observed is that which may be denied. Knowing there certainly is; known, the knowing certainly is not.

I may observe, or be aware of, a color, an odor, or any other sensation (sense datum); I may be aware of relations and feelings; I may be aware of any combination of these; but, Stout to the contrary notwithstanding, I am never aware of an awareness.

The possible objection to the statement just made, and probably

<sup>1</sup> See for example, in addition to the authors above quoted, Calkins, 'Psychology' (1910), 6-8. Myers, 'Experimental Psychology' (1909), 3-5. Pillsbury, 'Essentials of Psychology' (1911), 6-9; 'Attention' (1908), 212-217. Royce, 'Outlines of Psychology' (1903), 16-18. Titchener, 'Text-book of Psychology' (1909), 15-25. G. E. Moore, 'The Nature and Reality of Objects of Perception,' *Proc. Aristot. Soc.*, N. S., VI. (1905-6), 102-104. None of these authors explicitly presents a theory of introspection, so that we cannot say positively that they agree with Stout.



the logical foundation of the 'introspection'-hypothesis, is as follows: If one is not aware of awareness, he does not know that it exists. If one denies that he is ever aware of a thing, and that any one else is ever aware of it, he has no right to say that there is such a thing. The force of this argument is purely imaginary.

It may sound paradoxical to say that one cannot observe the process (or relation) of observation, and yet may be certain that there is such a process; but there is really no inconsistency in the saying. How do I know that there is awareness? By being aware of something. There is no meaning in the term 'awareness' which is not expressed in the statement "I am aware of a color (or what-not)."

So much for the logical foundation of 'introspection'; there is however a psychological reason for the rise of the theory. So many psychologists would not have assumed the reality of 'introspection,' if there were not some process or operation which simulates it. This process, I think, may be readily pointed out.

When one observes some 'external' object, as for instance sound, there are simultaneously present a number of other objects which are intimately connected with the observing of the sound, and which may not be themselves observed clearly. The muscular sensations from the tympanum, neck, breast, and other regions; the visual 'images'; the feelings; the visceral sensations; all these are definitely modified in the listening for the sound, and yet may not be vivid. On the other hand, the attention may be turned to these accessory facts, and the importance of the auditory sensation may be secondary. In this case, there seems to be a turning of the attention from the 'outer' fact (the sound) to the 'inner' facts. These facts are 'inner' in that they concern, or are constituents, of the body, or objective self. By a rather natural step, accordingly, these inner facts are taken to be the process of observing the sound. Observation of them is therefore the process of observing the process of observing the sound—*introspection*.

Stated in detail, this sort of 'introspection' is quite clearly the observation of things which are just as objective, considered from the point of view of knowledge, as is the sound; the trouble comes from the fact that we are apt to omit detailed statements. The double distinction between the subject and the object and between the self and the not-self, almost inevitably leads, in the absence of rigid analysis, to the identification of the objective self with the subject, and hence the vague conclusion that processes associated with the knowing of external objects are processes of *knowing* the same objects.

In actual practice, most psychologists who use the term 'introspection' and define it as the observation of consciousness not only do not seek to apply it in strict accordance with the definition, but they even apply it to the whole range of psychological observation. In giving 'introspective reports' on the observation of a sound, for example, the sound itself is usually included as one of the 'introspected' details. So colors, odors, after-images, and all other objects of consciousness, are quite commonly said to be 'introspectively' observed. This practice constitutes effectively the *reductio ad absurdum* of the 'introspection' theory. Starting as a distinctive kind of observation, the observation of an observation of something, it finishes as the only kind of observation. In other words, there would seem to be really nothing to observe except the observation of something else!

There is, as a matter of fact, not the slightest evidence for the reality of 'introspection' as the observation of 'consciousness.' Hence we must, in default of such evidence, cease the empty assumption of such a process. We might keep the word to apply to the processes we have described above (observation of feelings, and of kinesthetic and cœnesthetic sensations); a term by which to designate the observation of these factors would be very useful, and 'introspection' is the legitimate term for the purpose, since these factors are the real 'inner' ones of which psychology has been talking for so long a time; but in view of the word's quite disreputable past it is probably better to banish it for the present from psychological usage.

KNIGHT DUNLAP

THE JOHNS HOPKINS UNIVERSITY

NOTE.—After the foregoing discussion was placed in the hands of the Editor, Professor Titchener's interesting 'Prolegomena to a Study of Introspection' appeared in the July number of the *American Journal of Psychology*. Professor Titchener discards the Hamiltonian doctrine of the mind being 'self'-conscious in every cognition. What he substitutes for this doctrine is not made altogether clear, but apparently it is a theory similar to that of Stout or else (and this is more probable) the scholastic doctrine. This is indicated by such things as the implicit application of the term 'introspection' to the observation of sounds (p. 436), the statement that the psychologist 'is observing his own mind' (439), and the statement that 'introspection is the interrogation of experience' (440). The strongest indication is the contention that 'introspection' is not

necessarily a conscious process (442 *et seq.*). This doctrine, which at first seems highly paradoxical, is quite intelligible if we remember that 'consciousness' in Professor Titchener's mind-scheme is made up of 'processes' which are by no means to be identified with cognitions of objects, but rather with objects cognized. It is quite consistent with this terminology to say that 'introspection' is not primarily a 'conscious process'; it is the observation of a conscious process.





# THE PSYCHOLOGICAL REVIEW

---

## THE NATURE OF PERCEIVED RELATIONS

BY KNIGHT DUNLAP

*The Johns Hopkins University*

### § I. THE GENERAL PROBLEM

I shall attempt here to justify a certain way of considering perceived relations; a way which is not new, but which is radically incompatible with prevalent psychological theories, and which may therefore be expected to find little favor. In order to explain the view as fully as possible it is necessary to examine certain other views held on the perception of relation and to compare them with the one in question.

Differences in general psychological theory, as well as differences in terminology, render it almost impossible to state the positions of various psychologists in any common terms. Who can confidently determine, for example, the relations of the "Psychic Processes" of Wundt's *Outlines* to the "Modes of Being Conscious" and "Mental Processes" of Stout's *Manual*, and to the "Experiences" of Calkin's *First Book*? I shall therefore ascribe definite meaning to the statements of contemporary writers in a very few cases only, where the meaning seems to me to be unmistakable. In other cases I shall interpret very tentatively, or refrain from interpreting altogether.

I do not now intend to discuss "imageless thought," but am willing to concede, for the sake of argument, that relations are not perceived in temporal disjunction from sensible factors. Furthermore, I have no present concern with "forms of thought" or "perception qualities." Although the examination of these is well worth while, it has no essential place in the

consideration which I am here undertaking. As for "Bewusstseinslagen" and similar fabrications of the Germans; these terms have so little definite meaning that we would do well to exclude them and translations of them from English psychology.

No one, so far as I am aware, not even Bradley, really disputes the statement that relations exist, and are perceived.<sup>1</sup> No one, I suppose, would be less ready to challenge the statement that red and blue are apprehended than the statement that differences (in hue or in brightness) are apprehended. The only differences of opinion in regard to the perception of relations seem to be about (1) the nature of the perceived relations and (2) the nature of the perceiving of the relations.

Let us examine the statement that *relations exist*. By *relation* I mean such a thing as *difference* or *intermediacy*. Thus, green differs from red, and from blue, and each of these differs from blue-green. These differences are so definite that the difference between green and blue-green can positively be said to be less than the difference between green and red. Furthermore, the blue-green is in a relation of intermediacy to the blue and the green; a relation which may not be a new element, but may be merely the complex of the specific differences involved.

By existence, as applied to a relation, I mean exactly what I mean by the same term as applied to a sensible.<sup>2</sup> The green,

<sup>1</sup> The possibility that the essential 'mental' difference between man and the lower animals lies in the perception of relations by man, and the non-perception of relations by animals, is worthy of consideration. The results of the experiments on the learning processes of animals rather favor this view. It may be that animals, or certain animals, are so constituted that, for example, they are not aware of a difference between red and green successively seen, although they may react differently to the red and to the green, and to green preceded by red and green not preceded by red. The colors may be perceived, and may be different, and may provoke different reactions, and yet the difference not be perceived.

<sup>2</sup> *Sensible* seems to me to be the least ambiguous term in present use for the simple object of sense perception: the objective meaning of 'sensation.' Professor Bain uses *sensum*, to which there are several objections, the confusion in sound between *sensa* and *senses* being the practical one, and the logical implication of the past participle the most important. *Sentiendum* would be a better term. Alexander (*Proc. Arist. Soc.*, X., 1909-1910, p. 7) and Wodehouse (*The Presentation of Reality*, 1910, p. 60), use Bain's term; but Wodehouse also (p. 12) uses *sensatum* (!)

I shall use *sensible* where I distinctly mean the 'sensible quality.' In the historical survey, however, I am compelled to use 'sensation' even when it is clear that authors



the red, and the difference between them, are alike factual. This statement does not necessarily involve the assumption that in all cases the relations are exactly what they are judged to be. I may (possibly) have a fallacious awareness of a relation. The judgment that two reds differ in intensity may not mean, in a certain case, that the intensities of the two sensibles actually differ. On the other hand, it may be that a relation is whatever it is perceived to be.<sup>1</sup> Nor do we imply, by the affirmation of existence, that variations in degree of relation are of the same order as variations in degree of sensibles.

The grade of reality assigned to relations is therefore such as will account for the facts that (1) they function in complexes with sensibles, and (2) they are perceived along with sensibles. To this grade they are as a matter of fact admitted by practically every psychologist, as will appear. Our business now is to investigate the nature of these relations.

The various theories concerning the nature of perceived relations have been involved with theories of the nature of the perception of relations to such an extent that the two topics must perforce be considered together, the desirable separation being made subsequently, in so far as it is possible to make it at all. The theories may be reduced to six types, which are: (1) the sensational theory; (2) the scholastic theory; (3) the representative theory; (4) the kinesthesia theory; (5) the theory of relational states of consciousness; and (6) the theory of relational elements in content. To some of these types there are indefinite subtypes. They are not of equal logical rank; but the division is justified by its practical convenience.

have meant by it 'sensible,' since a change of terminology often changes an argument. Perhaps in the course of time we shall be able to use 'sensation' for *sentiens* alone, but for the present the term, while not so promiscuous in its connections of meaning as is 'mental,' is nevertheless so loose that it ought to be excluded from scientific usage as far as possible.

<sup>1</sup> I am assuming that relations really exist, whether perceived or not, if the sensibles which might be perceived in the relation, exist. When blue, blue-green, and green are presented to a subject who does not perceive the intermediacy of the blue-green with regard to the other two, they are nevertheless in the same relation of intermediacy as when it is perceived. This assumption may however be rejected, without damage to the exposition.

## § 2. THE SENSATIONAL THEORY OF PERCEIVED RELATIONS

Relations are said to be sensations. This is the theory which Thomas Brown accuses Condillac of holding.<sup>1</sup> It is true that these, and all other apparently non-sensational 'states of mind' are called by Condillac 'transformed' sensations; but the adjective, as Brown points out, does not materially alter the case. No matter how sensations are 'transformed,' they are still either (1) sensations or else (2) no longer sensations, but *ipso facto* something different in kind from sensations, in which case the statement is not as to the nature of sensations (which is by implication *sui generis*), but is merely as to their origin. Only one who holds to the first alternative is properly to be classed as a sensationalist in regard to relations.

What Condillac held concerning perceived relations is probably not what Brown understood him to mean, but merely the commonplace representative doctrine. Since no modern writer, so far as I am aware, upholds the strictly sensational theory<sup>2</sup> (a theory which would indeed tax the ingenuity of any one who should attempt to defend it!), we may legitimately ignore it.

## § 3. THE SCHOLASTIC DOCTRINE OF PERCEIVED RELATIONS

Scholasticism and representationism are so closely related in their treatment of relations, that it is sometimes difficult to determine which theory a certain writer comes the nearer to.<sup>3</sup>

<sup>1</sup> Brown, 'Lectures on the Philosophy of the Human Mind,' Lecture XXXIII.

<sup>2</sup> By sensationalism in regard to relations, I understand a theory which makes perceived relations out to be sensibles; not a theory which merely affirms them to be *intuitively perceived*. In order to be classed as a sensationalist, therefore, one must hold that relations are sensations of one or more of the accepted modalities, or must identify them with a new mode, comparable to the accepted modes of vision, audition, etc.

Titchener, for example, *if* he really holds definite relations to be nothing more nor less than definite kinesthetic sensations, or complexes of kinesthetic sensations (I do not accuse him of so holding), is certainly a sensationalist.

James defines sensationalism as the doctrine that no 'feelings of relation' exist, and therefore that no relations are actually perceived. I know of no evidence that any one ever held such a stupid doctrine; certainly Hume and Bain, the only two whom James specifically names, are clearly and emphatically not guilty.

<sup>3</sup> See Maher, 'Psychology,' Chapter IV., § The Scholastic Doctrine of Species; and Chapter VI.

By 'scholastic doctrine' I mean here the particular doctrine of Thomas Aquinas,

Nevertheless, when clearly stated, the two theories are distinct, and many psychologists are so explicit that we can decide which doctrine they hold.

The scholastic theory holds that sensation is an activity of the mind, which, on a somewhat low plane, cognizes an extramental reality. Sensation is not primarily an object cognized but is the *sentiens* of the object. Scholasticism is therefore sharply distinguished from representationism, in its doctrine of sensation. Relations, according to scholasticism, are cognized by an activity which is not only higher in degree than is the activity of sensation, but is also higher in kind.

"We fix upon a certain attribute of two or more objects, and comparing the objects pronounce them to be alike or unlike in this feature. This judgment is evidently distinct from the sensation or image of either object, though it presupposes sensations or images of both. It implies, in fact, a mental act distinct from the related impressions by which the relation subsisting between them is apprehended in an abstract manner. To affirm that the taste of a certain claret is like that of sour milk, or that the earth resembles an orange, there is required in addition to the pair of compared ideas a superior force which holds them together in consciousness, and discerns the relation of similarity between them. Neither the mere coexistence, nor still less the successive occurrence of the two impressions, could ever result in the perception of a relation between them, unless there be a third distinct activity of a higher kind to which both are present, and which is capable of apprehending the common feature."<sup>1</sup>

"*Intellect*, in its old and proper signification, as a higher rational activity superior to sense, awakened, indeed, to exercise by the latter, but transcending its range . . . makes intelligible the stream of change disclosed in sensation. . . . Now in normal perception these sensuous and intellectual elements are closely interwoven, and it may require careful attention and which has survived as the official theory of the Catholic Church. On this doctrine, I assume the statements of Maher, in his 'Psychology,' and in his article 'Intellect' in the *Catholic Encyclopedia*, to be conclusive. This is commonly called the 'intellectualist' doctrine, and opposed to a vaguely conceived 'Sensationalism.' See James, *Principles of Psychology*, I., 244-245.

<sup>1</sup> Maher, 'Psychology,' Chapter XII., § Comparison and Judgment.



reflection to separate them; but none the less are they radically different in kind."<sup>1</sup>

"Even in simple sensations, such as those of sight, there is genuine psychical activity of a certain kind; for the mind truly reacts to the physical stimulus by a conscious state. Still, compared with thought, sensuous life is relatively recipient and passive."<sup>2</sup>

Angell, in his *Psychology*, especially in the chapter on "The Consciousness of Meaning and the Formation of Concepts," takes the scholastic ground in regard to the perception of relations. The keynote of the chapter named is given in the statement that "The mind shows itself from the very outset as a relating activity."<sup>3</sup>

It seems reasonable to conclude that a 'mind' capable of the 'higher,' or intellectual, activity, is capable of submitting the lower, or sensational, activity to the higher, and so cognizing both it and its relations, as well as the relations in the external world. In its doctrine of the intellect, scholasticism has therefore not only a consistent account of the perception of real relations, but it has also a logical basis for the doctrine of introspection; a basis sadly wanting in systems which deny the 'higher' activity.<sup>4</sup>

In its doctrine of a mental activity which 'mirrors' the external world, scholasticism is approached rather closely by the representationism which it despises, or at least by the simple kind of representationism which grew directly out of it. For even if the sensational activity be primarily not representative, but intuitive; when it is introspected or cognized by the intellect it becomes a representation, although not a copy, of the external reality which, as a mental activity, it cognized. So too, Stout's doctrine of perception, and James's doctrine, although primarily intuitive, become representative, and explicitly so, through the function of introspection.<sup>5</sup>

<sup>1</sup> Maher, *op. cit.*, Chapter VII., § Intellect Usually Ignored.

<sup>2</sup> Maher, *op. cit.*, Chapter XIV., § Thought an Activity.

<sup>3</sup> Angell, 'Psychology,' 4th ed., Chapter X. An older work which takes the same attitude, although strongly influenced by Brown's representationism, is Abercrombie's 'Inquiries Concerning the Intellectual Powers,' especially in Part III., Section 4.

<sup>4</sup> See Maher, *op. cit.*, Chapter XI., § Reflexion and Self-consciousness.

<sup>5</sup> See my discussion of "The Case against Introspection," *PSYCHOLOGICAL REVIEW*, 1912, Vol. XIX., pp. 406-410.

#### § 4. THE REPRESENTATIVE DOCTRINE OF THE PERCEPTION OF RELATIONS

The theory of relation-perception to the consideration of which we now turn, I call the representative theory, because it is founded on, or rather is a part of, the representative theory of perception to which I have already alluded. According to this doctrine, which apparently has its modern beginning in Descartes, nothing extra-mental is experienced: the things experienced are either the mind's own modifications (the 'simpler' representationism), or something which somehow gets into the mind (the 'complex' representationism),<sup>1</sup> and in either case represent (not necessarily resemble) the external reality which is believed to exist, but to be perceived only in this odd way.

The rise of representationism was due to the demand that psychology be put to the practical test, as well as to the logical test. Psychology may be said to have become experimental in the hands of Descartes. The scholastic doctrine affirms the possibility of observing two distinct things: the external reality, and the sensuous activity in the mind which 'mirrors' that reality, or at least 'mirrors' certain attributes of that reality. If one puts this doctrine to the trial of observation, and finds not two things, but only one, he is apt to conclude that he either does not observe any external reality, or that he does not observe any internal process. For Descartes and for many since his time, the first alternative has seemed the more reasonable. Neither the empiricism of Berkeley, nor the idealism of Fichte, nor the realism of Reid succeeded in seriously hampering the progress of representative dualism, and with the advent of the so-called 'New Psychology' it became the official metaphysics of psychology. Some psychophysicists take it straight; some prefer it with a scholastic flavor; some qualify it with a brief statement of a quasi-Spinozistic belief that ultimately the mental modification and the external thing are the same; but nearly all, especially the experimental psychologists, accept it.

Representationism, being the child of scholasticism, starts its housekeeping with practically the same sort of mental

<sup>1</sup> The distinctions as drawn by Hamilton, 'Metaphysics,' Lecture XXI.

furniture as that found in the maternal house. The mental content of the latter is limited to sensations and feelings: the lower activities of the mind with their relations, which are objects of knowledge for the higher activity. So representationism assumes that the mental modifications, or mental processes, or ideas, which the mind may know, are to be completely accounted for as sensations (with copies, or images of these) and feelings. And like scholasticism, it assumes that the mind cognizes these contents and their relations (or that these somehow get cognized or experienced). The perception of the relations among the mental objects is assumed as an ultimate fact, just as naïvely as the physicist assumes the ability to compare 'physical' objects.<sup>1</sup> The business of the psychologist, in so far as relations are concerned, is apparently limited to the examination of the conditions under which sensations (and feelings) may be compared<sup>2</sup> with possibly an attempt to classify relations.<sup>3</sup> This latter procedure, I suspect, is considered by contemporary psychologists as an unprofessional infringement on the rights of logic.

It might be held that not all relations of mental states are perceived directly, but only the relations of certain classes of

<sup>1</sup> It is not worth while to pile up specific citations on this point. Any text-book of psychology (with the exceptions of those herein mentioned as taking a different view), opened in any chapter, will do. The exposition of Weber's law, in the ordinary text-book, assumes the perception of relations with great explicitness, and the usual exposition of Fechner's formula assumes even the perception of *differences between differences*.

<sup>2</sup> Bain, for instance, in 'The Senses and the Intellect,' in the pages preceding Chapter I. of *The Intellect*, says: "The transition from one impression to a second gives the consciousness of difference or discrimination; the occurrence of likeness in the midst of change yields a new and distinct effect, the effect of agreement." Bain's representationism is tintured with scholasticism: see *op. cit.*, Introduction, Chapter I., 1, where he assumes the cognizance of external reality. Yet James instances Bain as a typical "sensationalist" in regard to relations.

<sup>3</sup> The classifications of Locke, Hume, Abercrombie, Brown, and Upham are given at the end of this paper for their historical interest. Locke, 'Essay,' Vol. I., Book II., Chapter XXV. (especially § 8); Book II., Chapter XI.; and Book IV.; makes it clear that perceived relations are ultimate, and on a higher plane than the 'simple ideas of sensation.' Hume, although treating relations at considerable length in the 'Treatise,' Book I., Part I., §§ 4, 5; and Part III.; is less clear than Locke. He seems to have abandoned the scholastic doctrine of the perception of relations on a plane higher than that of sensation, but certainly is explicit as to their perceptibility and ultimacy.



states; the relations of the others might well be perceived indirectly, or representatively, through the relations of the favored classes. This is a rather simple extension of the fundamental principle of representationism, and might be based on any class of sensations, although kinesthetic sensations are the only ones which actually seem to have been supposed to function as the universal bearers of perceived relations. This theory is given a separate place below, under the heading of "the kinesthesia theory."

The representative treatment of relations seems logically consistent with the main doctrine of representationism. One who holds to that doctrine, it seems to me, must deny the existence of 'mental states' of relation, or else involve himself in embarrassing absurdities. The known content is constituted by 'modifications,' or 'processes,' or 'states' of the 'mind,' which are brought about, or in some way correspond to, certain external things. Now, two of the 'states' may be different, just as the objects they represent are different; but the difference between these 'states' can not possibly be a third 'state,' any more than the difference between iron and steel can be a third metal. If the 'mind' may be modified in one way, and then in another way, and if these ways are really different, and if each of these is perceptible without the aid of any other 'state,' so is their difference, or any other relation whose conditions are given by the two 'states' themselves. If there should be a 'mental state' of difference, it would necessarily include the two impressions so related, or else it would not be the particular difference between them; hence it could scarcely be elemental, but rather would be a specific combination of the two impressions.

So far, I have been discussing the simpler form of representationism. I do not see that the case would be essentially different with the more complex form. Assume that the furniture of the mind is something injected into it from without, instead of being carved out of the 'mind' stuff itself, and what I have said seems to apply. This problem is not very acute, however, for complex representationism, if it ever existed in a definite form, is not fashionable today. I have been merely

trying to show that the admission or rejection of relational elements of content, on the representative hypothesis, at least in its simpler form, is a definite problem, which does not especially concern those who have not accepted the creed of representationsim.

## § 5. THE KINESTHESIS THEORY OF THE PERCEPTION OF RELATION

The most reasonable construction to be put on the statements made by Professor Titchener, in the Division headed "Thought" in the Second Part of his 'Text-Book of Psychology,' is that he holds that the only relations directly perceived are those subsisting between, or 'carried' by, kinesthetic sensations. His statements are, however, not very definite, and I shall not insist that this is what he intended to express, but shall discuss the theory suggested by his statements as being possibly what he holds.

"In the author's experience, the feelings of relation are never simple. They are ordinarily matters of motor empathy; the relation is acted out, though in imaginal rather than in sensory terms. Sometimes the kinesthetic images are accompanied by a visual image, itself usually symbolic; sometimes they are strongly colored by pleasantness-unpleasantness. Whenever the relation is conscious, it is indubitably a complex of the familiar elements. But the path of habit leads, here as elsewhere, from the conscious to the unconscious; a relational word may switch one's ideas into a new direction, without any traceable representation of the relation within consciousness. It is, then, conceivable that the imageless relations of the text mark a halfway stage between kinesthetic set and unconsciousness; that, in certain individuals, a faint glow of consciousness still plays about excitatory processes which, in other individuals, are altogether unconscious. It is more probable that a systematically controlled introspection will, in all cases of consciousness, reveal the imaginal character of the feeling of relations."<sup>1</sup>

Now this might possibly be said to be a statement which

<sup>1</sup> Titchener, 'Text-book of Psychology,' § 140.

does not concern relation *per se*, but which concerns merely the doctrine that a relation can appear alone in consciousness: that one can think of (or perceive) a relation without any terms related;—a doctrine which Professor Titchener is in fact principally engaged in combating in the portion of his book to which I refer. The statement quoted, however, occurs in a section headed “The Alleged Elementary Process of Relation,” which opens with a specific reference to the James theory. Moreover, we are promised in Part I. (§ 10): “These relational processes we discuss in Part II.”: and the discussion from which I have quoted, and of which the quotation is fairly characteristic, is the only one which corresponds to the promise. It is regrettable that Professor Titchener has not been more explicit, and has not gone into detail with regard to the perception of relations as well as to relations in thought; but we must be content with his statements as they are.

Let me again remind the reader that I am not supporting the doctrine of ‘imageless thought’ or any other doctrine of the perception of bare relations (or of the perception of bare sensations either). In so far as Professor Titchener claims that the relation in thought is always acted out, in terms of some sort, his results seem to me unquestionable. Moreover, I admit that his analysis of ‘conscious attitudes’ into kinesthetic ‘sensations’ reduces these things into the only terms in which they could possibly be intelligible. But this does not touch the problem of the perception of relations, except to intensify it.

Suppose we fall back, for an example, on our old friend difference. If, when I am conscious (in thought) of difference between  $a$  and  $b$ , it is because the difference is acted out by kinesthetic ‘sensations’ or images, it must be that one kinesthetic complex functions for  $a$  and another for  $b$ . We have therefore in addition to  $a$  and  $b$  the kinesthetic complexes  $a'$  and  $b'$ ; and if the difference between  $a'$  and  $b'$  represents the difference between  $a$  and  $b$ , and the differences between the members in a certain number of other pairs,  $a_1b_1$ ,  $a_2b_2$ , etc.; then it is quite conceivable that the recurrence of  $a'$  and  $b'$  may represent this generalized difference, without any of the terms



to which the difference ultimately applies being present. But this is not possible unless the difference between  $a'$  and  $b'$  is perceived; and hence this process of kinesthetic abstraction is in no way the explanation of the perception of difference.

It is to be presumed, since the discussion from which I quote is of the 'alleged elementary process of relation,' that the kinesthetic processes are involved, not only in the quasi-imageless thought of relation, but in the case also where the terms related are clearly present in consciousness. Hence the construction which we must put on the theory involves the conclusion that the difference between two color 'sensations,'  $a$  and  $b$ , can not be perceived, but that each evokes a motor response,  $a'$  and  $b'$ , and that the difference between these motor responses is perceived, passing, in consciousness, as the difference between  $a$  and  $b$ . For, if no difference can be perceived between two 'sensations,' the adding of any number of new 'sensations' or images could not conceivably help the situation, unless some were finally added which permitted their difference to be noticed. A man might react in one way to one 'sensation' and in another way to another, but if he perceived difference neither between the primary 'sensations,' nor between the kinesthetic 'sensations' accompanying them (*i. e.*, the 'sensations' due to the reaction), he would perceive no difference. Animals may be thus limited in their experience, but we are not. It is the fact that we do, somehow, experience relations, which permits us to talk about them and to theorize as to how they are perceived.

An apparent concession to relations other than those of kinesthetic 'sensations' is made by Professor Titchener in his treatment of *meaning*,<sup>1</sup> in which he asserts that meaning is nothing but "context," which may be "carried in imaginal terms" although "originally, meaning is kinesthesia." In this section, I understand the author to mean that the representative function, in regard to relations, which he treats in other places as belonging to kinesthetic sensations and images only, may, by a process of substitution, come to be carried on by imagery of other sorts. This does not however materially alter

<sup>1</sup>Titchener, *op. cit.*, 103.

the situation. A present sensation, let us say, *means* a situation which occurred in the past through the arousal of an image drawn from that past situation: but this is possible only if the image *means* the past situation; otherwise it is merely one more item in the present object.

The kinesthetic theory, assuming that such a theory is actually held, requires an amount of evidence which has not yet been produced. The fact, if proved, that kinesthetic sensations or images accompany all our thoughts of relation, and all our perceptions of relations, proves nothing.<sup>1</sup> Moreover, why should we stop in the midst of a theory? If the seeming relations between green and red are supposed to be relations between complexes of kinesthetic sensations, or any function of kinesthetic sensations, why not assume that the green and the red also are really only kinesthetic complexes?

Any theory of relations which bases them on motor processes is an inversion of the normal order of horse and cart. The only assignable reason for the fact that small differences in stimulus produce large differences in reaction is found in the fact of the perceived differences in the sensations. A difference in sensation which produces no important difference in reactions while it is unnoticed, may, when noticed, produce a maximal difference. There is no direct correspondence between degree of sensation difference and degree of reaction difference. Suppose some one sorting different colored beads, and rejecting all which differ slightly from a given standard. If the standard be yellow, a blue bead may be rejected a little more quickly than a slightly greenish one; but the slight difference in time is inconsiderable in comparison with the difference between the keeping and the rejecting at all. My point here, however, is that a person who has had little training in observ-

<sup>1</sup> I am inclined to believe that Professor Titchener's finding only kinesthetic sensations when he looks for relations between other objects, is a proof (although none was needed) that he is an exceedingly acute and careful *introspector*. I have pointed out in a previous paper (*Psychol. Rev.*, XIX., p. 411) that 'introspection,' divested of its mythological suggestion of the observing of consciousness, is really the observation of bodily sensations (sensibles) and feelings (feelables). A psychologist who searches introspectively for the 'carriers of relations' between external objects is quite as likely to find kinesthetic sensibles as a policeman searching a colored boarding house is to arrest an afro-american.

ing color differences will keep beads which another, more expert (or the same person after more experience), will unhesitatingly reject. Now this training is not the association of some particular reaction with the colors or the color differences, for the person in question will discriminate just as well between beads *a* and *b* if he is selecting the class in which *a* belongs as if he were rejecting that class.

In all this, I may be whacking at a man of straw; there may be no one who holds to a kinesthetic theory of relation-perception such as I have been discussing. But if some of those who have been talking in a vague way about the motor processes in perception will take me seriously enough to say just what they mean, I will count my time well spent. It is possible that some who talk in terms of kinesthesia are really sensationalists; that they mean that a perceived relation *is* a kinesthetic sensation or a kinesthetic complex. It is clear that Professor Titchener, however, is not promulgating any such crude theory.

## § 6. THE THEORY OF RELATIONAL STATES OF CONSCIOUSNESS

This theory was elaborated by Thomas Brown, and was by him given a form very similar to that under which it is now widely known. Spencer and James took it directly over from him, although each of them modified it, for worse or better. Brown was rated by Hamilton as something of a bungler in philosophy, and he has been fairly well neglected for the past fifty years. Perhaps to a certain extent Brown merited the reputation he has received: it was surely stupid of him to erect his theory of 'feelings of relation' on the foundation of thorough-going representationism which he laid,<sup>1</sup> for the two are highly incompatible. Nevertheless, the development of this theory alone is sufficient to demonstrate Brown's genius, and to give him an honored place in the history of psychology.<sup>2</sup>

<sup>1</sup> Brown, 'Lectures on the Philosophy of the Human Mind,' XXVII. and XXVIII.

<sup>2</sup> James mentions Destutt (de Tracy), Laromiguière, and Cardaillac as among those who have "explicitly contended for feelings of relation" ('Principles,' I., 248). These philosophers were contemporaries of Brown; and Hamilton accuses Brown of having borrowed largely from Destutt ('Reid's Works,' 868). On this point at least, Brown can hardly be guilty of theft, for Destutt does not seem to have held a theory like that



After examining at some length the theory of relations which he alleges to be Condillac's, Brown states his own theory very clearly. "It is not, therefore, as being susceptible of *mere sensation*, but as being susceptible of *more* than sensation, that the mind is able to compare its sensations with each other. We may see, and certainly do see, objects together, without forming uniformly the same comparison; which could not be the case if the mere coexistence of the two perceptions constituted or involved the comparison itself. In the case of a *horse* and *sheep*, for example, though these, in the sensations which they excite, cannot, at different times, be very different, we compare, at different times, their color, their forms, their magnitudes, their functions, and the uses to which we put them, and we consider them as related in various other ways. The perceptions being the same, the comparisons, or subsequent feelings of relation, are different; and though the relation cannot be felt but when both objects are considered together, it is truly no part of the perception of each." A clear statement, even if erected on false premises. "Innumerable objects may be, and are, continually present to us *at once*, so as to produce one complex affection of mind,—fields, groves, mountains, streams,—but the mere *coexistence of these*, so as to form in our thought one scene, involves no feeling of comparison; and if the mind had not been susceptible of other affections than those of sense, or of mere remembrance of the past objects of sense, either in whole or in part, it might, when such a scene was present, have existed forever in the state which forms the complex perception of the scene, without the slightest notion of the *relation* of its parts to the whole, or to each other.

of Brown (James to the contrary notwithstanding), but rather an ordinary form of representationism. The theory of Laromiguière concerning relations is like that of Brown, but as Brown's 'Lectures' were written in 1810–11, and Laromiguière's 'Leçons de philosophie' were published between 1815 and 1818, there is no evidence of 'borrowing' here. Cardaillac (a pupil of Laromiguière) did not publish his 'Études élémentaires de philosophie' until 1830. The common features of the French and Scottish branches of the school must doubtless be attributed to the common antecedents. In giving credit to Brown, I am not belittling Laromiguière (whose system is in many respects superior to that of Brown), but I am moved by the consideration that the latter had no influence on English psychology, whereas the former has been profoundly influential—too much so.

"When I thus attempt to prove, by so many wearying arguments, that the feeling which constitutes our comparison of our sensations, or in other words, our belief in their agreement or disagreement, is itself a state of mind, different from either of the separate sensations which we compare, and different from both, as merely coexisting, I cannot but feel, what many of you have probably felt already, as if I were laboring to demonstrate a mere truism."<sup>1</sup>

The occurrence of a feeling of relation, Brown calls *judgment*, or *relative suggestion*. He says that comparison, "though it involve the feeling of relation, seems to me also to imply a voluntary seeking for some relation, which is far from necessary to the mere internal suggestion or feeling of relation itself. The *resemblance* of two objects strikes me, indeed, when I am studiously comparing them; but it strikes me also, with not less force, on many other occasions when I had not previously been forming the slightest intentional comparison." "When the feelings of relation seem to us to arise spontaneously, they are not in themselves different from the feelings of relation, that arise, in our intentional comparisons or judgments."

"I perceive, for example, a horse and a sheep at the same moment. The perception of the two is followed by that different state of mind which constitutes the feeling of their agreement in certain respects, or of their disagreement in certain other respects. I think of the square of the hypotenuse of a right-angled triangle, and of the squares of the two other sides;—I feel the relation of equality. I see a dramatic representation; I listen to the cold conceits which the author of the tragedy . . . gives to his hero in his most impassioned situations;—I am instantly struck with their unsuitableness to the character and the circumstances."

Brown devotes seven lectures to the details of 'relative suggestion.'<sup>2</sup> In the first of these lectures he reiterates very pointedly his systematic view of feelings of relation. "The *feelings of relation* are states of the mind essentially different from our simple perceptions, or conceptions of the objects that

<sup>1</sup> Brown, *op. cit.*, Lecture XXXIII.

<sup>2</sup> Lectures XLV-LI.

seem to us related, or from the combinations which we form of these, in the complex groupings of our fancy. . . . There is an original tendency or susceptibility of the mind, by which, on perceiving together different objects, we are instantly, without the intervention of any other mental process, sensible of their relation in certain respects, as truly as there is an original tendency or susceptibility of the mind, by which, when external objects are present, and have produced a certain affection of our sensorial organ, we are instantly affected with the primary elementary feelings of perception; and, I may add, that, as our sensations or perceptions are of various species, so there are various species of *relations*;—the number of relations indeed, even of external things, being almost infinite, while the number of perceptions is, necessarily, limited by that of the objects which have the power of producing some affection of our organs of sensation.

“The more numerous these relations may be, however, the more necessary does some arrangement of them become.”

Brown proceeds, accordingly, to arrange relations, classifying them under the two main heads of *relations of coexistence*, and *relations of succession*.

By means of the ‘feelings of relation,’ Brown solves the problem of universals, and puts conceptualism on a psychological (or pseudopsychological) basis. The universal is simply a relation of resemblance, to which a certain name is given. “The word *animal*, for example, is a general term, expressive of a particular relation of resemblance that is felt by us. *A horse is an animal*, is a *proposition*, which is merely the brief expression of this felt resemblance of a horse to various other creatures, included by us in the general term. It is the same in all the other species of relations which we are capable of feeling.” Then, specifying position, degree, proportion, and comprehension: “In all such cases, it is very evident, that the verbal statement of the proposition does not alter the nature of the relative suggestion, or feeling of relation, which it expresses, but simply expresses to others, a relation that must have been felt, before the proposition could be framed.”<sup>1</sup>

<sup>1</sup> Brown, *op. cit.*, Lecture XLVIII.



Statements similar to those quoted above from Brown occur over and over in his Lectures. It is clear that he considered the 'feeling of relation' a representative state of the 'mind'; quite on a plane with 'simple perceptions,' but generically distinct from them. The 'perceptions' represent external objects, which act on the 'sensorial organs'; the 'feelings of relation' represent the relations between the objects. Both are 'states of the mind.' Occasionally, Brown smuggles in real relations where he should be speaking of 'feelings of relation,' as in the last quotations above, but on the whole he is as consistent as any representationist.

In addition to the general difficulties in the way of a representative doctrine of relational elements, Brown introduces more, by his claim that the 'feeling of relation' and the 'simple perception' differ in kind. How two modifications of one thing *can* differ in kind, he does not explain. The difference must be nominal, like the 'transformation' of the sensation in the doctrine to which Brown so specifically objects, and to which, after all, his own scheme is somewhat akin.

Brown's philosophy was profoundly influential in Great Britain and in America in the first half of the nineteenth century. Thomas Upham, for example, in his 'Elements of Mental Philosophy,' published eleven years after Brown's 'Lectures,' shows the influence of the latter work explicitly, and follows Brown's treatment of relative 'suggestion,' classifying the relations on a scheme modified very slightly from that of Brown.<sup>1</sup> Abercrombie also adopts Brown's 'relative suggestion,' although not his classification.

## § 7. JAMES'S VERSION OF THE THEORY OF RELATIONAL STATES

James's ideas on the perception of relations show clear indications of having been taken from Brown, but they were made more workable in the scheme of James. James maintains that there are 'feelings of relation' as well as 'sensations' but he does not insist so strongly on the difference in the kind of 'mental state'; the 'state' moreover is not that which is

<sup>1</sup> Upham, 'Elements of Mental Philosophy,' Volume I., Division First, Part Second, Chapter IV.; or, *abridged edition*, Part II., Chapter IV.

known, but is the knower. James is primarily a realist, and falls back on a kind of secondary representationism only to account for memory and introspection. The 'state of consciousness' knows an external reality<sup>1</sup> and knows its own bare existence,<sup>2</sup> and these are direct experiences; but when the external reality is no longer present, the 'state' which knew that reality is known by the 'state's' successor, and by virtue of the first 'state's' having previously known the object, it represents the object now when it itself is known.<sup>3</sup>

Some of the 'states' know the qualities of objects, and others (or else the same 'states') know relations. "*So surely as relations between objects exist in rerum naturâ, so surely, and more surely, do feelings exist to which these relations are known.*" "If we speak objectively, it is the real relations that appear revealed; if we speak subjectively, it is the stream of consciousness that matches each of them by an inward coloring of its own. In either case the relations are numberless, and no existing language is capable of doing justice to all their shades."<sup>4</sup>

The feelings here spoken of are 'substantive states,' or rather, the 'substantive state' may be analytically considered as if it included 'feelings of relation' and sensations; actually, it is a simple 'state' which is aware of a complex object involving relations: for James quotes with approval Brown's remark on the mistake "Of supposing that the most complex states of mind are not truly, in their very essence, as much one and indivisible as those which we term simple,"<sup>5</sup> and on the next page says that "*There is no manifold of coexisting ideas*; the notion of such a thing is a chimera. Whatever things are thought in relation are thought from the outset in a unity, in a single pulse of subjectivity, a single psychosis, feeling, or state of mind." Later, he says: "We, on the contrary, put the Multiplicity with the Reality<sup>6</sup> outside, and leave the mind sim-

<sup>1</sup> James, 'Principles of Psychology,' I., 216-220; II., 3-6.

<sup>2</sup> *Op. cit.*, I., 341.

<sup>3</sup> *Op. cit.*, I., 336-342.

<sup>4</sup> *Op. cit.*, I., 245.

<sup>5</sup> *Op. cit.*, I., 277.

<sup>6</sup> *Op. cit.*, I., 363.

ple." The 'feelings of relation' so far described are therefore on the plane of sensations in at least one important respect: neither really exists, but the 'feeling of relation' is the limit which the 'state' approaches as the sensible qualities in the object known become fewer, and the 'sensation' is the limit as the relations in the object become fewer. This is made more certain in the explanation of the connection between sensation and perception, where we are told that "pure sensation" is the state cognizing a "simple quality apprehended irrelatively to other things" and that "the fuller of relations the object is, . . . the more unreservedly do we call the state of mind a perception."<sup>1</sup>

Although James's doctrine of perception is based on the theory of immediately perceived relations, in the chapter on Space he is somewhat confusing. While admitting that "Most 'relations' are feelings of an entirely different order from the terms they relate" he insists that "*In the field of space the relations are facts of the same order with the facts they relate.*"<sup>2</sup> In a footnote, however, he crosses his trail by admitting that "All that we have meant by speaking of up and down, right and left, as *sensations*" is expressed by saying with Kant that they are matters of 'immediate intuition.'<sup>3</sup>

Not only in this case, but in others, there are inconsistencies in James's 'Principles,' which are readily explained. The various portions of the work were written at different times, and not in chapter order, and hence the terminology and the mode of expression vary. Perhaps James's theories varied too, slightly. It may be that when he wrote the chapter on the Perception of Things, for example, he was inclined towards ordinary representationism; but we are not warranted in assuming this by the lapses into representative phraseology.

I have said that according to James, certain relational 'states' are 'substantive.' This is not in accord with the ordinary interpretation of James's theory, but has the virtue of being more in accordance with what James says than is the

<sup>1</sup> *Op. cit.*, II., 1.

<sup>2</sup> *Op. cit.*, II., 149.

<sup>3</sup> *Op. cit.*, II., 151, footnote.



opposed view, which assumes that the 'feelings of relation' are identical with 'transitive states.'<sup>1</sup>

It is manifestly impossible that the relations which are known with the related objects in 'one unitary pulse of thought' should be known by a 'transitive state,' while the said objects are known by a 'substantive state,' for the same 'state' can not be both 'transitive' and 'substantive'; neither can it be said that the difference between a 'transitive state' and a 'substantive state' is a mere analytical fiction. The distinction is real, and the two are separated in time, as well as distinguished by difference in rate of change. Clearly the 'feelings of relation' are not all 'transitive states': and James explicitly says they are not. Discussing the relation theory of Spencer, he says: "His philosophy is crude in that he seems to suppose that it is only in transitive states that outward relations are known; whereas in truth space relations, relations of contrast, etc., are felt along with their terms, in substantive states as well as in transitive states, as we shall abundantly see."<sup>2</sup> This statement is as plain as could be, and as it was apparently written after the main part of the chapter with which it occurs, there is no doubt that James meant it; and the principle involved in it is fairly well adhered to throughout the book.

What are the 'transitive' relational 'states' in James's system? Their significance is made clear by the consideration of the nature of the 'states of consciousness.' In the analogy of the kaleidoscope<sup>3</sup> James is nominally describing the brain process, but really has in mind the 'psychosis,' as is shown by his skip from the 'lingering' brain process to the 'lingering consciousness.' The stream of consciousness here, as in the whole chapter, is supposed to be something (really mind-stuff

<sup>1</sup> Woodworth is one of the few who have written concerning James's theory and have not overlooked the substantive relational states. See "The Consciousness of Relation" in the volume of essays in honor of Professor James by members of the Columbia Faculty, 485-507. Woodworth holds to the main points of James's theory, but extends it to affirm the perception of bare relations;—of relations without any terms related; and also holds that all qualities may be resolved into relations.

<sup>2</sup> *Op. cit.*, I., 248-249, footnote. It is rather odd that Calkins ('Introduction to Psychology,' 134) should accuse James of the very thing for which James condemned Spencer.

<sup>3</sup> *Op. cit.*, I., 246.

of some sort) which is in constant change; but there are moments in which the change is relatively slight, separated by moments in which it is large, like the successive rearrangements of a kaleidoscope which is slowly rotated. The 'relatively stable states of consciousness' are perceptive of objects (including sensible qualities and relations); but if these 'states' are cognitive, the intervening 'states' (for they certainly are 'states') must also be cognitive. Cognitive of what? On this point the theory is not absolutely clear, but it seems to be that the cognition *may* be of some relation between the two things cognized by the temporally adjacent 'substantive states,' although the cognition is certainly in some cases cognition of the transition from the one object to the other. It seems almost as if James meant by 'transitive states' the 'relations of succession' of Brown. The 'feeling of tendency' is apparently a 'transitive state' in which the importance of the 'state' *from* which is slight and the importance of the 'state' *to* which is large.

I may illustrate the difference between the 'substantive' and 'transitive' 'feelings of relation' by the cognition of bread-and-butter in the first place, and the cognition of bread followed by the cognition of butter in the second place. I may be aware of the bread and the butter as a complex object; that is, my 'state' at a given moment may be aware of the bread, of the butter, and the relation of the one to the other. Or, I may be aware of the bread, and then aware of the butter; the transition from the one 'state' to the other *may* be the 'feeling of *and*,' as when I am taking stock of the different articles on the table before me. These two complexes, bread-and-butter on the one hand, and bread *and* butter on the other, are essentially different; one involves Brown's 'relation of coexistence,' and the other a 'relation of succession.'

The 'state' cognizing bread may however be separated from (or connected with, *comme il vous plaira*) the 'state' cognizing butter, by a 'feeling of *but*,' as when I expected or desired some bread without butter, and find it spread. In this case, the 'state' does not cognize merely a relation existing *in rerum naturâ* between the bread and the butter (if indeed the 'feeling

of *and*' did), but a very complex relation between the bread-and-butter and the bread-*sans*-butter cognized by a preceding 'state' (imaginatively). We might indeed question whether the 'states' under discussion should not really be given the greater part of their individuality through the emotional contents they cognize; but this is like asserting that an author ought to have made a character to be otherwise than as it is actually delineated.

We have noticed the fact that the 'substantive' 'feelings of relation' are as unreal as 'sensations'; the transitive 'feelings of relation' on the contrary are as real (in the mechanical scheme, that is) as the actual 'substantive states.' No wonder that the 'transitive' 'feelings of relation' are the ones which had the main interest for James, so that we are in danger of forgetting his other variety.

If James's theory of the *observing* 'mental state' is clever, his theory of the *observed* 'mental state' is a marvel of ingenuity. The 'mental state' observed (in memory) represents, as we have said, the objects (including relations) which it previously knew; but, in the passage from subjectivity to objectivity the 'transitive states' disappear, and only a series of 'substantive states' is left for scrutiny.<sup>1</sup>

By the ingenious device of the disappearing 'transitive state' James makes his machine go, whereas Brown's contrivance, lacking this device, breaks down. James does not attempt to make an objectified mental modification function as a relation between two other mental modifications. In his representative system he has only the 'states' standing for the relatively distinct objects; and presumably the new 'state' which knows these is also aware of the relations between them (not mental modifications, but real relations between mental modifications), and these relations may therefore represent the relations between the objects represented by the 'states.'

Two points are left unexplained, it is true. The analysis in memory of the unitary 'substantive states,' which are in actuality neither 'feelings of relation' nor 'sensations,' ought

<sup>1</sup> See my discussion of 'The Case against Introspection,' *PSYCHOL. REVIEW*, 1912, XXI., pp. 408-409.



in conformity with the doctrine in regard to the 'transitive states' to show no 'feelings of relation,' but only 'sensations.' But whether this is the solution James intended, or whether he would have said that after all the analysis is not of the 'state' but of the object represented thereby, we can not say. The second point is how any 'state,' even the most 'substantive' can survive in memory at all, since really none of them endure, but are at every moment in change, the difference between the 'substantive' and the 'transitive' being only a matter of degree, not of kind. But such minor criticisms have really no point at this date. It is a fine old model of a 'mind' that James constructed, and the fact that nothing like it has ever been discovered on the earth or in the heavens above the earth or in the waters that are under the earth ought not to prevent our giving due admiration to the nice adjustment of its wheels and levers.

The theory as I have explained it is the one presented by James in his *magnum opus*. In his later writings, after he rejected not only the 'feelings of relation' but the whole camorra of 'feelings,' he still contended for the reality of the cognized relations, dealing mostly with the relations corresponding to the 'transitive states,' and talking in terms of 'experiences.'<sup>1</sup> 'Consciousness' is abandoned, and the function of cognition is carried on between 'experiences'; one 'experience' knows another and is known by a third. Just what these 'experiences' are, James does not explain, but the term does not have either of the significations which it has in the 'Principles,' and I can find no definite meaning which will apply.<sup>2</sup> Probably it is merely one of the pawns in the meta-

<sup>1</sup> 'The Thing and its Relations,' *Journal of Philosophy, etc.*, 1905, Vol. II., 29-41; 'A World of Pure Experience,' *Journal of Philosophy, etc.*, 1904, I., 522-543, 561-570; 'Does Consciousness Exist?' *Journal of Philosophy, etc.*, 1904, Vol. I., 477-491; 'The Meaning of Truth,' 1909.

<sup>2</sup> These 'experiences' are not actual experiences of objects, as the term is legitimately used; they are not cognitions, for they cognize one another, and no other objects for them to cognize are provided; to speak of one cognition cognizing another reminds one of the old story of the people who lived by taking in one another's washing. To suppose that the 'experiences' are objects cognized, reduces the situation at my breakfast table to the coffee-pot experiencing the toast, and the toast experiencing the butter, and the butter experiencing a feeling of satisfaction; which renders it rather

physical game which James was playing in his latter years. The only certain point in James's last works is that he knew that the details into which he had worked out his radical empiricism were wrong, but that the direction in which he was working was right. This is more than it is given to many philosophers to know, and we could not expect that in his last years he would be able to rebuild his system with his former precision.

A theory of relation-perception which is possibly somewhat like that of James has been propounded by Professor Calkins. At least, as stated in her first text-book,<sup>1</sup> it seems to be like James's theory. But in her second psychological text<sup>2</sup> the situation is rendered doubtful by her ascription of theories similar to hers to Ebbinghaus and Münsterberg, who are not generally understood as favoring James's theory. Another source of difficulty in the interpretation of Professor Calkins's doctrine of 'relational elements' is in her peculiar use of the term 'elements.' Intensity and extensity, for example, are in her system not points with regard to which elements may differ, but are themselves elements. Extensity is "an entirely peculiar kind of feeling."<sup>3</sup> Moreover, while the 'consciousness' which Miss Calkins discusses may be something like that which James invented and subsequently discarded,—there is at least an indication in the early part of the First Book that it 'knows' something extra-mental,—the author is constantly reverting to a different usage, as when she 'concentrates' her 'consciousness' on the boat,<sup>4</sup> and even to the commoner usage, in which 'consciousness' or 'experiences' are considered as if they were the objects seen, smelled, etc.<sup>5</sup>

needless for me to attend the function at all. Some realists may have such a scheme as this to propose, but as James was blest with a sense of humor it is not fair to assume that he was preaching this doctrine.

<sup>1</sup> Calkins, 'Introduction to Psychology,' Chapter X.

<sup>2</sup> Calkins, 'First Book of Psychology,' Chapter VIII., II., and Appendix,' § VIII., II.

<sup>3</sup> Calkins, 'First Book,' Chapter III., I., a, 3.

<sup>4</sup> Calkins, *op. cit.*, Chapter VI., I.

<sup>5</sup> Calkins, *op. cit.*, Chapter III.

## § 8. THE CONSIDERATION OF RELATIONS AS ELEMENTS OF CONTENT

If we give up the attempt to stage the experienced world as a drama of 'mental modifications,' 'mental processes,' 'states of consciousness,' or any other marionettes of the theater called the 'mind,' and try to find, by observation, what it is that we observe, and into what elements or factors it may be by observation resolved, and how the elements or quasi-elements and the complexes or quasi-complexes behave, we may make some progress. We find sensibles (*sentienda*) and we find that these are not absolutely simple, even in our analysis, but that they differ from one another, or vary in condition, in several specific ways. If we compare two sensibles such as red and green, we notice a difference in *quality*; and the comparing, in this case, is nothing but the noticing of the difference. If we compare two sweets,<sup>1</sup> we may notice another difference, the difference in *intensity*. When we talk abstractly about the elementary sensibles, we speak of the fact that they may differ in these characteristic ways by ascribing to them *characters* of quality and intensity. We can not conceive of a sensible which has not these two characters (as well as certain others); that is to say, we can not conceive of a sensible which does not involve these two (and certain other) relations. By this, in the last resort, we mean no more than that we never observe sensibles which are not found involved with these relations, if the observation has been thorough.

If on the other hand we 'attend to' the relations, we find them always involved with sensibles. No difference is perceived, except a specific difference; by specific differences we mean differences which are ultimately differences of definite sensibles. We may, of course, have differences of differences; we may have differences of other relations; relation of all sorts, in short, may be related; but the differences which differ reduce finally to differences of sensibles, and all other relations of relations rest, even if at several removes, on a sensible foundation.<sup>2</sup>

<sup>1</sup> Assuming that there are two sweets possible. If not, the wording of the statement needs a slight change.

<sup>2</sup> I do not mean to say that the relations are *internal*; on the contrary, I am con-



Admitting feelables (as pleasure, pain) without argument, we have three sorts of elements out of which the known reality may be considered as made: relations, sensibles, and feelables. These are all that we observe; we do not observe observing or observation; hence we can not say that the observing of one of these three classes (that is, the consciousness of one of them) differs from the observing of either of the others; we can not say, therefore, that there is any difference between sensation and feeling, or between either and the consciousness of relation. We are not supposing any different kinds of consciousness, but only different kinds of things of which we are conscious. And of these, even, we are not conscious separately, but are conscious of complexes in which all three coexist; or rather, we may resolve the perceived objects into these elements, if the two statements really mean different things.

The first English writer to state the general result of this analysis, so far as I know, and possibly the first to state it in any language, was Berkeley. The statement was not embodied in the bishop's earlier writings, nor indeed in the first edition of any of his writings, but was added to the second edition of his 'Principles' in 1734, after his works had all been published. It represents therefore, without doubt, his mature and final view. It is hardly ever mentioned by critics and expositors, although it puts a new face on his system; but as Fraser has reminded us, those who have commented on Berkeley have rarely thought it necessary to read him carefully first.

"We know, and have a notion of relations between things and ideas—which relations are distinct from the ideas or things related inasmuch as the latter may be perceived by us without our perceiving the former. To me, it seems that ideas, spirits, and relations are all in their respective kinds the objects of human knowledge and discourse; and that the term idea would be improperly extended to signify everything we know or have a knowledge of."<sup>1</sup>

vinced that they are *external*, although I lay no stress on this point at present as it does not seem essential to the establishment of the reality of perceived relations. See Russel, 'Philosophical Essays,' 160-169; or 'The Nature of Truth,' *Proceedings of the Aristotelian Society*, 1906-7: also James, 'The Thing and its Relations,' *Journal of Philosophy, etc.*, II., 36-38.

<sup>1</sup> Berkeley, 'On the Principles of Human Knowledge,' 2d edition, § 89. See also § 12 and § 142.

In Berkeley's terminology, of course, the 'idea' is not the consciousness of anything, but anything not included in the categories of relations and spirits, of which one is or may be conscious. An 'idea' is spoken of by Berkeley as being 'in the mind,' but by that phrase, as he carefully explained, he meant no more than that the 'idea' was perceived. The 'mind' with which psychology has had so much alleged dealing in the last hundred years, Berkeley tried to abolish altogether. Far from holding that the world is 'subjective'; that the senses deceive us; or that the physical world is unreal; as is even now said of Berkeley; he held to the immediate perception of an objective world. In this world, relations are perceived as much as are sensibles. It is true that the 'ideas' of Berkeley really include relational elements; but although he did not carry his analysis down to the bottom, he recognized the nature of its results.

The main features of the consideration of relations as elements of content—what I should prefer to call the empirical theory of relations, if it may logically be called a theory—may be stated categorically under three heads.

1. Real relations of real objects are really perceived. The perceived world is real, and not a phantasm or species of some unperceived world. As the brown color of the table on which I write is a real color of a real table, so the difference I notice between the brown of the table-top and the black of the ink in the well, is a real difference.

2. The elementary things perceived—sensibles, relations, and feelables—are not parts or functions of the perceiving ego, any more than they are 'states of consciousness' by which something else is perceived. To say that these objects are 'mental' can mean only one of two things: (1) that the objects are perceived or may be perceived; or (2) that the Protean term *mental* is used to cover the whole universe. In the latter case, the distinction between the perceiving and the perceived remains the same, although both are called 'mental,' as if one were called 'mental' and the other 'non-mental.' But, if when we have defined experienced things, as 'mental,' we neglect the fact that we have extended the term, and proceed to treat the

objects as if they were 'mental' in the restricted signification, we plunge our psychology into grievous confusion. Better and simpler to abolish the terms 'mental' and 'psychical' for all scientific purposes.

3. The consciousness of one object does not differ in kind, so far as we know, from the consciousness of another object. The only differences (aside from a possible difference of degree or vividness) are in the objects of consciousness. Of differences in consciousness corresponding to the three kinds of elements of content we have absolutely no evidence.

4. Neither sensibles nor feelables nor relations, it is probable, are ever perceived alone. It can not be said with confidence that such perception is impossible, but in ordinary cases the consciousness is of an object which involves two and probably all three of these elements; or, the object may be analytically resolved into these.

From these statements it is clear that I am not advocating a theory of 'relational elements of consciousness,' and that I am not concerned with the question whether 'bare' relations are perceived. It is also clear, I hope, that I am willing to join in the spirit of the common saying (as voiced by James and Brown) that consciousness is *simple*, however much I may take issue with the mechanical schemes on which this simplicity has been interpreted. However complex the object may be, there is no possibility of analyzing the consciousness thereof. Hence, there is no discernible difference between the consciousness of one element in the object and the consciousness of another element.

It should also be clear that this empirical view of relations is developed on a basis which makes 'introspection' in the traditional sense impossible. This is a perfectly satisfactory basis, as there is no evidence that any one was able at any time to 'introspect.' Certainly the present writer is able to do it not one whit more than he can send out 'soul vibrations.'

In affirming relations to be elements of content, we are not complicating the current psychological scheme by adding something to it, but are really simplifying by eliminating superfluities. Current opposition to 'relational elements' seems to



be directed altogether at the theory of Brown, which I have admitted to be an impossible addition to representationism. We are not trying to add 'relational states' to 'sensational states,' but are subtracting the 'sensational states.' None of the 'states,' 'modifications' or 'processes' of the 'mind' are discoverable, and we would do well to discard the misleading labels. The only 'relations' we affirm are the actual ones, of which, I suppose, no one really has ever doubted. The things related are the feelables, and the sensible qualities (to use James's term) of real objects.

Certain modern philosophers, interested in the problem of knowledge from a non-psychological point of view, are in the habit of calling consciousness a relation: either the relation of subject and object, or (now that subjects are out of fashion) a peculiar relation of objects to one another.<sup>1</sup> This seems to raise a difficulty, for if consciousness can be asserted to be a relation, comparable in any way to other relations, then it is knowable. The difficulty, however, is an entirely arbitrary one. In the game of metaphysics, where each term is given its definite value and then played according to the fixed rules of logic, it is quite legitimate to define consciousness as a relation, or as an activity, or as a lamp-post; and then, if the definition is not forgotten in the heat of the game (as sometimes happens), the terms may be carried through with perfect consistency. In psychology, however, we are restricted by the rule that we must attempt to describe and analyse things as they seem really to be; and the consciousness of which we are speaking here is not comparable to difference, or to any other relation, any more than it is comparable to activity. Relations, activities, and lamp-posts are things which are known; and consciousness (as the term is here used) is the knowing which is

<sup>1</sup> Many writers use the term 'relation' to designate consciousness, but it is possible that some of them merely do so in default of a more suitable term, and would not strictly hold to the position to which the usage seems to commit them. Among those who are perfectly explicit on this point however are some of the most brilliant of living philosophers, whose opinions it seems presumptuous for me to challenge. For example: Professor Alexander, 'On Relations; and in Particular the Cognitive Relation,' 1912, *Mind*, N. S., XXI., 305-328. Professor Woodbridge, 'The Nature of Consciousness,' *Journal of Philosophy, etc.*, II., 119-12 and 'Consciousness, the Sense Organ and the Nervous System,' same *Journal*, VI., 449-455.

involved in that statement. If one defines consciousness as a relation (or a lamp-post), he thereby sacrifices a useful term needlessly, and continues to assume the same old consciousness, for which he needs a new name, in addition to the relation (or lamp-post), which he calls by the old name.

In conclusion, I wish to register the admission that neither the existence nor the perception of relations as perceived facts can not be proved in a strictly experimental way. Neither can the existence or perception of sensibles. You can no more prove that the difference between red and green is perceived by any one, than you can prove that green is perceived. These things are fundamental postulates, justifiable only by observation, on which psychological theory and experimentation are based. Assume the representative postulates, and you build a system of psychology which furnishes specific problems for experimental solution; but the interpretation of the experimental results is in terms of your postulates. Reject your postulates, and a great deal of what was supposed to be experimental results goes with them.

With the recognition of relations as real, a real psychology of perception and thought seems possible. So far we have contented ourselves (in spite of the efforts of the Würzburgers) with a mere psychology of sensibles, because we have assumed that relations were indispensable tools which we might work with, but which we should not presume to work upon.

#### CLASSIFICATIONS OF PERCEIVED RELATIONS.

The "connexions of and agreement, or disagreement or repugnancy" of ideas according to LOCKE:

- I. Identity, or diversity.
- II. Relation.
- III. Coexistence, or necessary succession.
- IV. Real Existence.

The "kinds of relations" according to HUME:

- 1. Resemblance.
- 2. Identity.
- 3. Space and time.
- 4. Quantity or number.
- 5. Degree of quality.
- 6. Contrariety
- 7. Cause and effect.

*Difference*, according to Hume, is a "negation of relation."

The "feelings of relation," "relative suggestions," or "relations either of external objects or of the feelings of the mind" according to BROWN:

- I. Of coexistence.
  1. Position.
  2. Resemblance or difference.
  3. Degree.
  4. Proportion.
  5. Comprehensiveness. [Whole and part.]
- II. Of succession.
  1. Casual.
  2. Invariable. [Cause and effect.]

The "relations of things, whether referring to external events or mental processes" according to ABERCROMBIE:

1. Character.
2. Resemblance and analogy.
3. Accordance. [Inductive.]
4. Composition.
5. Causation.
6. Degree and proportion.
7. Moral relations.

The "classes of relations" according to UPHAM:

- I. Identity and diversity.
- II. Degree.
- III. Proportion.
- IV. Place or position.
- V. Time.
- VI. Possession.
- VII. Cause and effect.

According to SPENCER, the "feelings of relation" are strictly *changes in consciousness*. Spencer distinguishes between *relations of succession* and *relations of coexistence*, but fundamentally all feelings of relation are relations of *succession*. The feeling of *coexistence* is a double change, between exactly the same terms in each case but in opposite directions. A single change, from one term to another, is the relation of *unlikeness* or *difference*, which is therefore the fundamental relation-feeling. *Likeness*, or *non-difference* is a double change between two terms, from the first to the second and back to the first, the two differences cancelling. Spencer does not explain how this differs from mere coexistence; perhaps he intended to identify it with likeness, or to include it in the latter.

There are two classes of differences: differences of *degree* and differences of *kind*. Finally, all relations of succession (which seems to mean *all* relations) are divided into the *fortuitous*, the *probable*, and the *necessary*.

Spencer, 'Principles of Psychology,' Vol. I., Ch. II., § 67; Vol. II., Chs. XIX.-XXIV.



# THE EFFECT OF LENGTH OF SERIES UPON RECOGNITION MEMORY<sup>1</sup>

BY EDWARD K. STRONG, JR.

Much work has been done in studying the effect of length of series upon recall memory. The results differ considerably in different investigations but they all go to show that as the length of the series increases there is a much greater corresponding increase in the time or energy necessary for its mastery. The purpose of this paper is to present the results obtained in a corresponding study in which, however, *recognition* memory was employed. The results so obtained indicate that the number in the series of stimuli affects the results almost in direct proportion to the increase. That is, as the number of stimuli is increased the per cent. that can be recognized decreases.<sup>2</sup>

Recognition memory refers in this connection to the process of recognizing or identifying an object as having been seen before in some particular connection. No reason for the identification need be present,—all that is necessary is that the individual is certain that this is the object which he encountered before. The difference between *recognition* and *recall* memory is then that the former requires simple identification of the object when it is presented a second time while the latter requires a reproduction of the object without any external aid whatever.

## THE EXPERIMENT

The experiment was as follows. Six series of advertisements were used consisting of 5, 10, 25, 50, 100, and 150 advertisements each.<sup>3</sup> The advertisements were exposed at a

<sup>1</sup> From the Columbia University Psychological Laboratory.

<sup>2</sup> As will be shown later this generalization is not quite true, but it is exact enough for a general statement.

<sup>3</sup> As different advertisements have very different attention-value two series of 5 advertisements were actually employed so as to minimize the effect of the particular

uniform rate of one per second. Immediately after the advertisements in any series had been presented a second set containing those advertisements and an equal number of other advertisements was given the subject. He was then instructed to pick out those advertisements from the second set which he recognized as having been just previously shown him. The subject was further instructed to sort this second set into four piles. In the first pile he was to put only those advertisements he was *absolutely* sure he had just seen (100 per cent. sure); in the second pile he was to put those advertisements he was *reasonably* sure he had just seen but of which he was not absolutely sure (about 75 per cent. sure); in the third pile, those advertisements which he had a faint idea he had seen but of which he was not at all sure (about 25 per cent. sure). The remainder of the advertisements,—those he did not remember seeing,—were to be put in the fourth pile. In this way the subject sorted the advertisements into three piles representing respectively 100 per cent., 75 per cent., and 25 per cent. surety, and into a fourth which represented rather lack of memory about its contents than positive knowledge that they had not been presented before.

The advertisements used in these series were all full-page advertisements and were all different except that the advertisements in the series of 150 comprised the advertisements in the series of 100 and of 50 advertisements. But no subject that was used in the study of the series of 150 advertisements was used with the series of 100 or 50 advertisements. In this way no advertisement was repeated for any subject. Extreme care was exercised also that no two advertisements advertising the same commodity and the same firm should be used. It was impossible to obtain enough advertisements so that no firm should be duplicated. But, as already stated, if two or more advertisements of the same firm were used it was seen to that they were of widely different commodities, such as the General Electric lamp and the General Electric flatiron. Unfortunately, after advertisements used in these very short series. The results, however, of the two series of 5 advertisements each were almost exactly identical. They are therefore combined into one result and reported as such throughout the paper.

the experiment had been carried on for some time, it was discovered that there were four or five advertisements of the same commodity and firm in the several series. A careful watch was made of the effect of these advertisements. They were mistakenly identified to a greater extent than the others but the effect on the totals given below is less than one half per cent.

Each series of advertisements was presented to 20 men and 20 women. All the subjects were students at Columbia University, and were all within a few years of each other in age. The same subject was shown not more than 4 series, so that the results are not obtained from 40 different subjects, but from a considerably larger number.

TABLE I

TOTAL NUMBER AND PERCENTAGE OF CORRECT RECOGNITIONS FOR EACH OF THE SIX SERIES

Sub- jects	No. of Ads.	Total Number			Percentage				
		Pile No. 1.	Pile No. 2	Pile No. 3	Pile No. 1	Pile No. 2	Pile No. 3	Summary	P. E.
80	5	4.11	.23	.11	82.3	4.5	2.3	86.2	1.4
40	10	8.15	.45	.25	81.5	4.5	2.5	85.0	1.3
40	25	18.23	1.20	1.15	72.9	4.8	4.6	77.6	1.8
40	50	30.80	3.00	2.50	61.6	6.0	5.0	67.4	2.0
40	100	56.40	6.60	4.80	56.4	6.6	4.8	62.6	2.2
40	150	60.60	10.50	6.90	40.4	7.0	5.6	46.8	1.7

## THE RESULTS

1. *Correct Recognitions.*—Table I. presents the data concerned with the correct recognitions. The first column states the number of subjects,—in each case half were men and half were women. The second column gives the number of advertisements presented in the series. The next three columns give the average number of advertisements which were correctly recognized in piles No. 1, No. 2, and No. 3, respectively. The next three columns give the same results but in terms of per cent. of the total number of advertisements presented. In other words, the first row of this table states that there were 80 subjects employed, that 5 advertisements were presented in the series, and that 4.11 of these were correctly recognized



by placing them in pile No. 1, .23 in pile No. 2, and .11 in pile No. 3. Putting these figures into per cent. we have 82.3 per cent. of the 5 advertisements placed in pile No. 1, 4.5 per cent. in pile No. 2, and 2.3 per cent. in pile No. 3. The score in the "summary" column was computed in the following manner. As pile No. 2 represented 75 per cent. certainty (not 100 per cent. certainty as in pile No. 1) only three fourths of the score in that column is credited toward the summary. Similarly, only one fourth of the score in the column under pile No. 3 is credited toward the summary, because here there was only 25 per cent. certainty in the minds of the subjects when they placed an advertisement in the pile. The summary column thus comprises the amount under pile No. 1, three fourths under pile No. 2, and one fourth under pile No. 3. There may be some objection to this procedure but it is manifestly fairer than to credit the results in the last two piles with full credit. All the subjects understood the scoring and sorted the advertisements on that basis. Preliminary experiments indicated very clearly that advertisements were recognized with varying degrees of certainty and that unless the subject clearly understood that his results would be credited on a sliding scale he would not pick out any but those of which he was absolutely sure.

A survey of the total percentages indicates very clearly that there was a steady decrease in the number of advertisements that could be recognized as the length of the series increased. (This decrease is shown more plainly in Plate I., where the data are plotted.) This per cent. decreased from 86 per cent. with 5 advertisements to 47 per cent. with 150 advertisements. The percentages from the three piles indicate that there was an even more pronounced decrease in those advertisements which are recognized with absolute certainty as the length of the series increases but that there was an opposite tendency in the second and third piles. Here there is a gain of from 2 per cent. to 3 per cent. in each pile as we proceed from series of 5 advertisements to series of 150 advertisements. But the amounts under these two piles are after all only a small portion of the total amount. It is evident,

then, that ordinarily we either recognize an object or we don't recognize it,—that recognition of 150 advertisements just seen from among 300 is little concerned with “maybes” and “perhapes.” But it is also evident, that as the difficulty of the

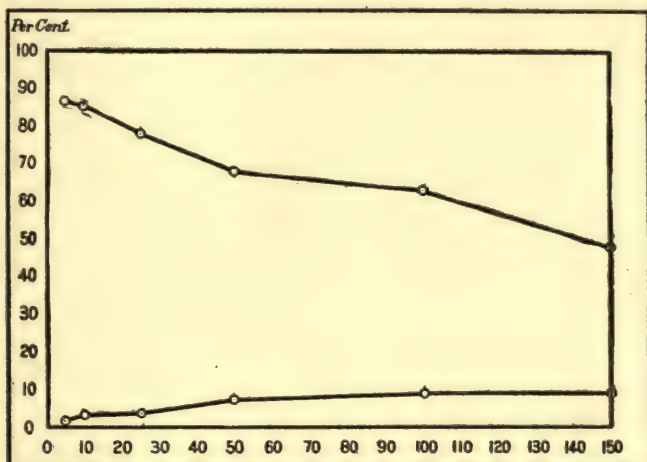


PLATE I. Showing (1) the Decrease in the Per cent. of Correct Recognitions and (2) the Increase in the Per cent. of Incorrect Recognitions as the length of the Series is increased.

task increases we have proportionately less and less certain recognitions.

2. *Incorrect Recognitions.*—But not all the recognitions that are made are correct ones. Some advertisements are identified as having just been seen that actually were not present at all. These facts are presented in Table II. and plotted in Plate I.

TABLE II

TOTAL NUMBER AND PERCENTAGE OF INCORRECT RECOGNITIONS FOR EACH OF THE SIX SERIES

Subjects	No. of Ads.	Total Number			Percentage				
		Pile No. 1	Pile No. 2	Pile No. 3	Pile No. 1	Pile No. 2	Pile No. 3	Summary	P. E.
80	5	.04	.03	.11	.8	.5	2.3	1.7	.3
40	10	.10	.13	.40	1.0	1.3	4.0	2.9	.5
40	25	.30	.28	1.28	1.2	1.1	5.1	3.3	.6
40	50	1.70	1.58	2.40	3.4	3.2	4.8	7.0	.7
40	100	4.53	3.58	5.40	4.5	3.6	5.4	8.6	.8
40	150	5.68	6.88	6.75	3.8	4.6	4.5	8.3	.7

A study of the total amounts or the percentages shows clearly that there was a slightly greater tendency to recognize falsely in the third pile than in the other two piles. This tendency to pick out wrong advertisements also increased with the increase in the length of the series. Indeed, the tendency to recognize falsely seems to increase approximately as the length of the series increases. But the surprising thing, after all, is that such a very small per cent. of wrong recognitions should be made, that, for example, when 40 per cent. of 150 advertisements are picked out correctly in the first pile only 4 per cent. of the new and incorrect advertisements should be identified as having been seen before. Increasing difficulty of the task results then in decided decrease of recognitions but it does not cause much of an increase in incorrect recognitions. Ability to know then that we haven't seen seems to be more strongly fixed than ability to pick out what we have seen.

3. *The Validity of the Recognitions.*<sup>1</sup>—It is interesting to note now just how accurate the recognitions were. When a subject put one advertisement in the first pile, another in the second, and another in the third, did he approximate at all closely to the designated percentages of 100 per cent., 75 per cent., and 25 per cent. certainty? Table III. and Plate II. supply us

TABLE III

ACCURACY OF THE JUDGMENTS FOR EACH OF THE PILES OF THE SIX SERIES. ALSO  
THE ACCURACY OF EACH PILE FOR EACH SERIES IS SHOWN IN TERMS OF  
RATIOS OF THE ACCURACY OF PILE NO. 1

Subjects	No. of Ads.	Per Cent. Correct			Ratios in Terms of Pile No. 1		
		Pile No. 1	Pile No. 2	Pile No. 3	Pile No. 1	Pile No. 2	Pile No. 3
80	5	99.1	90.0	50.0	100	91	50
40	10	98.8	78.3	38.4	100	79	39
40	25	98.4	81.4	47.4	100	83	48
40	50	94.8	65.6	51.0	100	69	54
40	100	92.6	64.9	47.1	100	70	51
40	150	91.4	60.4	50.6	100	66	55

with this information. We have there expressed the per cent. of advertisements which were correctly recognized in the three piles. That is, 99.1 per cent. of all the advertisements placed

<sup>1</sup> Validity of recognitions equals (number of correct recognitions)/(number of correct + incorrect recognitions).



in pile No. 1 in the experiment with a set of 5 advertisements had actually been shown previously while 0.9 per cent. had not been shown. We find here a very slow but steady decrease in the accuracy of the judgments as the length of the series

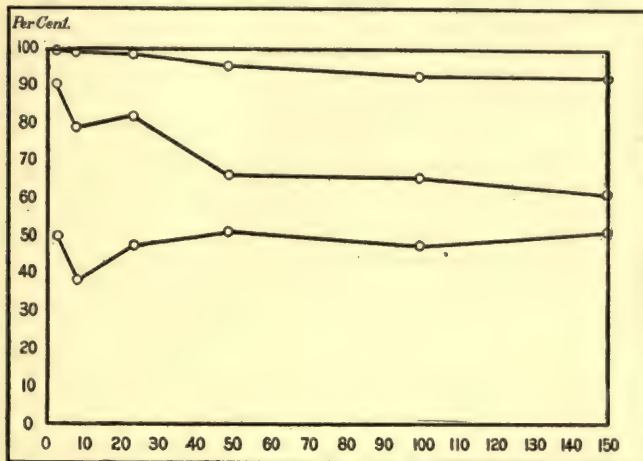


PLATE II. Showing the Relationship between length of Series and the Accuracy of the Recognitions for the three Grades of Certainty.

is increased, a decrease from 99.1 per cent. with 5 advertisements to 91.4 per cent. with 150 advertisements. Likewise we find a steady decrease in the accuracy of the recognitions in pile No. 2. But here the decrease is much more rapid as the series is lengthened, a decrease from 89.9 per cent. correct recognitions with 5 advertisements to 60.4 per cent. with 150 advertisements. On the other hand, the validity of the recognitions in pile No. 3 is not affected by the length of the series, as far as can be determined from this experiment. Here the percentage of right recognitions remains approximately 50 per cent. Pile No. 3 was meant to be used when the recognition seemed but slightly better than a guess, or one correct in four (25 per cent.). Under the conditions of this particular experiment, where we have an equal number of right and wrong advertisements from which to select, a pure guess will be, of course, 50 per cent. of the time correct. That the recognitions in pile No. 3 approximate 50 per cent. means simply that they are no better than random selections.

TABLE IV  
THE VALIDITY OF THE RECOGNITIONS FOR EACH PILE SHOWN BY THE DISTRIBUTION OF THE INDIVIDUAL RECORDS

	No. of Ads.	Percentages																					Total No. of Subjects	Median Position	
		100	95	90	85	80	75	70	65	60	55	50	45	40	35	30	25	20	15	10	5	0			
Pile No. 1.	5	39						I															40	99.9	
	10	36		4																			40	99.7	
	25	33	4	3																			40	99.5	
	50	15	18	4	2	I																	40	96.1	
	100	13	12	8	5	I																	40	94.6	
	150	10	10	10	5	4	I																40	92.5	
		6			I																		8	99.2	
Pile No. 2.	5	10																					14	99.0	
	10	19																					24	99.3	
	25	12						I															32	82.5	
	50	5	1	1	1	1	7	3	3	1	3	1	2	1	3	1							34	70.8	
	100	4	1	3	3	1	3	5	3	2	6	3	1	1	2	1							37	68.0	
	150																								
		3																					7	50.0	
Pile No. 3.	5	10																					13	48.8	
	10	5																					19	36.7	
	25	2						I															27	52.0	
	50	4																					30	51.7	
	100	2	1	1	1	2	2	5	1	1	1	3	3	3	2	2	1						35	51.0	
	150																								
		5			1		1	5	1	3	5	3	3	3	4	2	1								

The validity of the recognitions is set forth in another manner in Table IV. Here the individual subjects are considered. The table presents the distribution of the 40 subjects according to the accuracy of their recognitions. To the right of these is given the number of subjects and then the median position of the group. In this way one can grasp much more easily the slow but steady increase of mistaken recognitions in the first pile. This increase is not so easily seen in the results of the other two piles. This is due to the fact that there was a great difference in the number of individuals using these piles. The greater the length of the series the larger was the number of subjects who used them. There was, moreover, a corresponding increase in the number of advertisements put in these piles. But at no time did the number placed in piles No. 2 and No. 3 anywhere equal those placed in pile No. 1. This fact largely accounts for the scattered distribution in these two piles for when few advertisements are considered the percentages of right advertisements will vary much more than when many advertisements are considered. The medians representing the whole group, however, show with pile No. 2 the steady decrease in the validity of the recognitions and with pile No. 3 the approximation to 50 per cent., as already pointed out above.

The ratio between the validities of the three piles is given in Table III. where in each case the validity of the recognition in pile No. 1 is called 100. With the exception of the results from the set of 5 advertisements (where there were less than 10 subjects using piles No. 2 and No. 3) we have approximately three fourths the accuracy in pile No. 2 that we find in pile No. 1, and one half the accuracy in pile No. 3 that we find in pile No. 1. From these figures one might suppose that from an introspective standpoint the relation between pile No. 1 and pile No. 2 was maintained, *i. e.*, that although the actual results fell below the assigned standards, yet the relation between them was maintained. This point is upheld by the totals but a perusal of the distribution in Table IV. shows that it was not the case with the individual subjects. Although the writer does not feel that the data in this experiment are con-



clusive on this particular point, yet what data we do have point to the fact that the relationship of 100 per cent., 75 per cent. and 25 per cent. was not upheld, nor was there any constant relationship between the three piles. With each successive series, implying a difference in the difficulty of the task, the relationship between the three piles changed.

4. *Results Combining (1) the Number of Correct Recognitions and (2) the Validity of the Recognitions.*—At first thought it might be considered that the results presented in Table I. actually give the true relationship between length of series and the capacity to recognize. This would be so if all individuals took the same attitude of conservatism or optimism in selecting those advertisements they recognize. But this is not the case. Some will pick out only those advertisements they are absolutely sure of and will not use the second or third piles. The reason for their doing this is not entirely that recognition with them is either positive or lacking,—it may be partially due to this,—but it is due to a conservative temperament. My notes record a good many instances where such individuals have pointed out certain advertisements and exclaimed, "I think that was there," and yet put it in pile No. 4. If they followed instructions they should put it in pile No. 2 or pile No. 3 but they do not do so. They do not like to take chances. On the other hand, some individuals will take a chance on every advertisement, placing all those they are not absolutely sure of in piles No. 2 or No. 3 and not putting any in pile No. 4. As a general rule the conservative individual makes practically no mistakes in his first pile while the optimistic individual makes many. Now if we scored their results only in terms of the total number of correct recognitions, the latter's record would be much higher than the former's. On the face of it, such records are not true,—they do not represent the exact situation. It is for this reason that it is necessary to combine the two elements of amount of recognitions and their accuracy into one record. The method for doing this is as follows.

There are three factors which must be taken into account in obtaining a fair summary. There is first the number of advertisements that are correctly recognized,—the relationship

between the number recognized and the total number that should be recognized. There is second the accuracy of the recognitions,—the relationship between the number of correct and the number of incorrect recognitions. And there is third the general scheme of the experiment. In this experiment the subject had to select from an equal number of right and wrong advertisements. If this was changed so that he had to select from twice as many wrong as right advertisements the results would be different. In order to compare the results from two such experiments, the writer has multiplied the number of incorrect recognitions by such a factor as would equalize the chances between the right and wrong advertisements. For example, in the above case where twice as many wrong as right advertisements are presented to be chosen from, *i. e.*, when the chances are 1 to 2 in favor of selecting a right advertisement by chance, if the number of incorrect recognitions is multiplied by  $\frac{1}{2}$  we then have results of correct and incorrect recognitions which are comparable to those which would be obtained if the chances had been even at the start. Such a correction then takes account of the third factor which we are considering. No correction for this third factor,—the general scheme of the experiment,—need be made in this experiment as the chances of making a correct or incorrect recognition are already equal.

Turning now to the first factor we see at once that by reducing the total number recognized to per cent. of all that should have been seen we can compare directly the results from, say, a series of 5 advertisements with the results from any other series (just as is done in Table I.). Such a comparison is expressed by the formula,

$$\frac{\text{correct recognitions}}{\text{total number presented}}$$

In presenting the second factor we must recognize that when there is an equal chance of selecting a right or wrong advertisement (when an equal number of each are presented as in this experiment) a record of 50 per cent. correct recognitions means nothing but pure chance. This 50 per cent. correct recognition

really means then nothing else than *zero memory* for although the subject has picked out  $x$  advertisements correctly from the  $n$  advertisements presented originally yet he has picked out an equal number incorrectly from those advertisements which had not been presented to him. As far as it is possible to consider the question we must consider that the  $n$  advertisements presented first have made no impression on him else they could be distinguished from the equal number that were not presented. We may then call *zero memory* in this case the condition where an equal number of right and wrong advertisements are picked out or where more wrong than right advertisements are selected. Actually this condition is never even approximated in the first pile and is only occasionally obtained in the second pile. *Perfect memory*, on the other hand, would be, of course, where the  $n$  advertisements presented were all recognized and none of the wrong advertisements were selected. The following formula,

$$\frac{\text{correct recognitions} - \text{incorrect recognitions}}{\text{correct recognitions} + \text{incorrect recognitions}},$$

will give 100 under those conditions corresponding to perfect memory as defined above, 0 under those conditions corresponding to zero memory as defined above, and equal steps between the two extremes as the factors vary successively.

By combining the two formulas and multiplying the results by 100 to have it read in terms of per cent. instead of a decimal we have

$$\frac{\text{correct recognitions}}{\text{total number presented}} \times \frac{\text{correct recognitions} - \text{incorrect recognitions}}{\text{correct recognitions} + \text{incorrect recognitions}} \times 100.$$

This formula combines the per cent. of correct recognitions among the possible recognitions with the accuracy of the recognitions. Some examples of the use of the formula will make this clear. Consider 10 advertisements shown and then the subject asked to pick out those he recognizes as having just seen from 20 advertisements consisting of the 10 he has seen



and 10 new ones. Suppose (1) he picks out all of the 10 correctly. We have then perfect memory, or the following:

$$\frac{10 \text{ (correct recog.)}}{10 \text{ (total no. presented)}} \times \frac{10 \text{ (correct recog.)} - 0 \text{ (incorrect recog.)}}{10 \text{ (correct recog.)} + 0 \text{ (incorrect recog.)}} \times 100 = 100.$$

Suppose (2) he picks out 5 correct ones and 5 wrong ones. We have then,

$$\frac{5}{10} \times \frac{5 - 5}{5 + 5} \times 100 = 0.$$

And (3), suppose he picks out 5 correct ones and 1 incorrect one. We have then,

$$\frac{5}{10} \times \frac{5 - 1}{5 + 1} \times 100 = 33.$$

Having considered now the method for getting a true summary of amount of recognition and its accuracy let us apply the method to our present investigation. Table V. presents

TABLE V

RELATIONSHIP BETWEEN LENGTH OF SERIES AND RECOGNITION MEMORY

(The first column under each pile represents the per cent. of correct recognitions among the total possible number. The second column gives the validity of the recognitions, and the third column gives the product of the first two columns.)

No. of Ads.	Pile No. 1			Pile No. 2			Pile No. 3			Summary
5	82.3	98.5	81.1	4.5	60.9	2.7	2.3	0	0	83.1
10	81.5	97.8	79.7	4.5	51.8	2.3	2.5	.7	0	81.4
25	72.9	96.9	70.6	4.8	54.1	2.6	4.6	4.2	.2	72.6
50	61.6	90.3	55.6	6.0	32.8	2.0	5.0	3.3	.2	57.2
100	56.4	87.3	49.2	6.6	22.7	1.5	4.8	3.6	.2	50.4
150	40.4	83.4	33.7	7.0	25.6	1.8	4.6	3.3	.2	35.1

these results. In the first column under each pile is given the per cent. of correct recognitions among the whole number of possible recognitions. These figures are taken from Table I. and represent the results to be obtained from the first part of the formula. In the second column under each pile in Table V. is given the results which are obtained from the second part of the formula. The third column under each pile gives the product of the other two columns, which is again expressed in terms of

per cent. The summary is computed in the same way as are the ones in Tables I. and II., explained above. The data in this summary column are shown in diagram form in Plate III.

From this plate it is evident that the data all lie in a straight line except the results from the series of 25 and 50 advertisements. But as the probable error of the average of 50 advertisements is less than 2 it is extremely unlikely that its

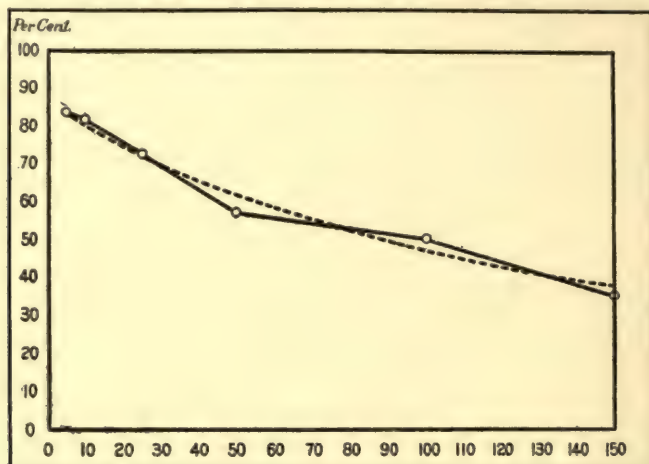


PLATE III. Showing the Relationship between length of Series and Recognition Memory (when amount and accuracy of recognitions are both considered).

true position lies in a straight line with the other figures, which would mean a displacement of ten or five times the probable error. The most probable "smooth curve" for the six points seems to be a curve similar to the one that is dotted in on the plate. This curve passes approximately through three of the six points and approaches fairly closely to the other three. If this is the true relationship, then, between length of series and recognition memory, we must conclude (1) that the per cent. of correct recognitions decreases more slowly than the length of the series increases and (2) that the rate of decrease is steadily decreasing as the length of the series increases.

Several other points of interest are suggested by Table V. The per cent. of correct recognitions in pile No. 1 decreases from 81 per cent. with a series of 5 advertisements to 34 per cent. with a series of 150 advertisements, in pile No. 2 the

decrease is from 2.7 per cent. to 1.8 per cent., while in pile No. 3 it is practically 0 per cent. throughout. Evidently then recognitions that are not accompanied with a feeling of "absolute certainty" are practically no better than random guesses. They are no better in pile No. 3 and only amount to 1.5 per cent. to 2.7 per cent. in pile No. 2, which represents "reasonable but not absolute certainty" or "75 per cent. certainty." This dearth of doubtful recognitions is due primarily to the fact that the subjects did not have such recognitions. Even in the series of 150 advertisements where we find the greatest number of them they only amount to 7.0 per cent. in pile No. 2 and 4.6 per cent. in pile No. 3 which together are only one fourth of the number in pile No. 1. But besides this dearth of doubtful recognitions there is also the fact that a very great number of those that were made turned out to be false recognitions. Practically none above those which would be expected from pure chance are found in pile No. 3. In pile No. 2 it is somewhat better. But even there we find only 33 per cent. more than what chance would warrant in the series of 50 advertisements and with the series of 100 and 150 advertisements the per cent. drops to 25 per cent.

#### MISCELLANEOUS NOTES

- (a) No sex difference of any importance was noted.
- (b) No correlation was found between ability in this experiment and "general intelligence" as indicated by (1) class-work and (2) the consensus of opinion of a subject's colleagues.
- (c) Memory span when the objects are presented in succession is an entirely different thing from memory span when the objects are presented simultaneously.

#### CONCLUSION

1. When 5 advertisements are successively exposed 86 per cent. can be recognized immediately after, while only 47 per cent. can be recognized from 150 advertisements similarly exposed.
2. The per cent. of correct recognitions decreases as the length of the series increases. (This decrease is possibly faster at first and then steadily becomes less as the series are increased in length.)



3. When 5 advertisements are successively exposed and then recognitions are made from those advertisements and an equal number of wrong ones 1.7 per cent. of the latter will be incorrectly recognized as having been among those originally shown. In a series of 150 advertisements 8.3 per cent. of the wrong advertisements will be incorrectly recognized.

4. The per cent. of incorrect recognitions increases as the length of the series increases. This increase is more rapid at first and then becomes less and less as the series are increased in length.

5. Practically, very few incorrect recognitions are made as compared with the total of correct recognitions. It seems then that the ability to know we have not seen an object is much more strongly fixed than the ability to pick out what we have seen.

6. The accuracy of recognitions decreases rather slowly with "absolutely certain" recognitions and rather rapidly with "reasonably sure" recognitions as the length of the series increases, while it approximates zero with "very doubtful" recognitions regardless of the length of the series.

7. A true measure of recognition memory must take into account not only the per cent. of recognitions made out of the possible number but also the relationship between correct and incorrect or mistaken recognitions. When these two factors are considered we obtain a curve from which we deduce (1) that the per cent. of correct recognitions decreases more slowly than the length of the series increases and (2) that the rate of this decrease steadily decreases as the length of the series increases.

8. Recognitions not accompanied with a feeling of "absolute certainty" are practically no better than random guesses.

9. As the difficulty of the task increases the ratio of "absolutely certain" recognitions to "reasonably certain" and "doubtful" recognitions decreases. The first two groups actually also become less and less accurate but this decrease in accuracy is very marked in the "reasonably certain" group. The "doubtful" recognitions are practically worthless at all times.

# THE EFFECT OF CHANGES IN THE GENERAL ILLUMINATION OF THE RETINA UPON ITS SENSITIVITY TO COLOR<sup>1</sup>

BY GERTRUDE RAND

*Bryn Mawr College*

- I. Introduction.
- II. Historical.
- III. Experimental.

- (1) Quantitative estimate of the influence of change of illumination upon the induction of brightness by the surrounding field.
- (2) The effects of these amounts of induction on the limits of color sensitivity.
- (3) The effect of these amounts of induction on the limens of color at different degrees of excentricity.
- (4) The influence of change of illumination upon the effect of the preëxposure on the limens and limits of color.

## IV. Conclusion.

### I. INTRODUCTION

It is the purpose of this paper to show the effect of changes in the intensity of the illumination of the field of vision upon the results of investigations which deal with the sensitivity of the retina to color.<sup>2</sup>

A method of standardizing the illumination of the field of vision was described in an earlier paper.<sup>3</sup>

### II. HISTORICAL

The effect of the general illumination of the retina on color

<sup>1</sup>From the Bryn Mawr Psychological Laboratory.

<sup>2</sup>With regard to this effect two cases may be recognized: (1) When the colored light used to stimulate the retina is independent of the general illumination, *e. g.*, when it is obtained from the spectrum, from monochromatic sources, or from standard filters; and (2) when it is obtained by reflection from pigment surfaces. In the first case the effect is exerted in the following ways: (a) by changing the brightness relation of the preëxposure to the colored surface, (b) by changing the brightness relation of the surrounding field to the colored stimulus, (c) by altering the sensitivity of the retina to brightness after-image and contrast and thus changing the effect of the brightness of the preëxposure and of the surrounding field upon the sensitivity of the retina to the colored stimulus. To these effects is added in the second case a change in the amount of colored light coming to the eye.

<sup>3</sup>Ferree and Rand, 'An Optics-room and a Method of Standardizing its Illumination,' *PSYCHOL. REV.*, 1912, XIX., pp. 364-373.

sensitivity has been recognized since the time of Purkinje and Aubert. It has been studied in some detail by a number of experimenters, among whom may be mentioned Kramer and Wolffberg. Both have shown that the sensation aroused by the colored stimulus is weakened by a reduction of the general illumination but neither has given a method of keeping the general illumination constant. Kramer's<sup>1</sup> purpose was to determine the sensitivity of the eye under different intensities of daylight and artificial illumination. His method was as follows. Stimuli, 4 mm. square, of blue, yellow, red, and green paper on a black background were used. The distance at which the stimulus had to be placed from the observer to be just recognized as colored was tested by sunlight and when the sky was obscured by clouds and for three intensities of each of the following sources of artificial illumination: candle-light, gas, petroleum, sodium, potassium, strontium, and calcium lights. His results show the following facts: (1) Red is seen at the greatest distance in all lights except calcium, in which case green is seen when placed farther away than red. The other colors are recognized in the order green, yellow, blue. (2) All the colors are recognized at a greater distance when seen by sunlight than when illumined by artificial light or the dull light from a clouded sky. (3) As the intensity of the artificial illumination is decreased, the colors must be placed nearer the eye to be recognized. In drawing his conclusions with regard to comparative sensitivity, Kramer ignored the white contrast which the black background induced across the stimuli. The induction across stimuli whose sizes were only 4 mm. square must have been considerable. It was, moreover, of different amounts in each case; because brightness contrast is greatest when there is maximal brightness opposition. The modification of the light colors, as a result of contrast induction, must, therefore, have been greater than that of the dark colors. Wolffberg's<sup>2</sup> interest was in the influence of gradual alterations of the general illumination on

<sup>1</sup> Kramer, J., 'Untersuchungen über die Abhängigkeit der Farbenempfindung von der Art und dem Grade der Beleuchtung,' Inaug.-Diss., Marburg, 1882.

<sup>2</sup> Wolffberg, 'Ueber die Prüfung des Lichtsinnes,' *A. f. O.*, 1887, XXXI., pp. 1-78.



the light and the color sensitivity of the central and of the peripheral retina. His room was illuminated by daylight entering through a window. Fifteen different degrees of illumination were produced by fastening from one to fifteen thicknesses of tissue-paper over the window. The illumination obtained when the window was uncovered was called 15/15; when covered with one thickness of tissue-paper, 14/15, etc. His method of determining the effect of variations of illumination upon the central retina was as follows: Pigment stimuli were placed at a standard distance of 5 meters from the observer, and the size of stimulus necessary to render it just visible in its true color was determined. In the peripheral retina, he investigated to what extent the limits of white and of colored stimuli were altered by reducing the illumination. In all his experiments, the stimuli were fastened on a black background. Wolffberg's results for the central retina are shown in the following table. The stimuli were circular in shape and of diameters given in columns 2, 3, 4, 5, and 6.

Illumination	Size of Red Stimulus	Size of Blue	Size of Green	Size of Yellow	Size of White
15/15	.5 mm.	3 mm.	3 mm.	1.5 mm.	.2 mm.
14/15	1.5	5	4	2	.5
13/15	2	6	6	4	1
12/15	2.5	12	12	4.5	2
11/15	3	20	20	5	2.5
.....	.....	.....	.....	.....	.....
5/15	10	50	50	10	6

These results show that in the central retina a decrease of illumination has greater effect upon the sensation of color than upon the sensation of white. Wolffberg next tested the effect of a gradual decrease of illumination upon the limits of sensitivity to white and to the colors. He found that the extent of the visual field was not narrowed for white when the illumination was decreased to 1/15. The color limits, however, narrowed gradually when the illumination was decreased from 15/15 to 3/15. The narrowing was in no case more than 15°. The relative extents of the fields remained unaltered, *i. e.*, the order of size was in every case blue, red, and green.

Although special investigations have been conducted by Kramer, Wolffberg, and others to show the effect of changes in the general illumination upon color sensitivity, in general little if any precautions have been taken by earlier experimenters to prevent such changes when investigating color sensitivity. Either the experimenter has not considered the influence of the general illumination, or he has been satisfied to take the rough precaution to work only on bright days at stated hours. Ole Bull,<sup>1</sup> for example, commented at length on the factor of general illumination, but suggested no method for its standardization. He writes: "The amount and nature of the general illumination are of more significance in perimetrical observations than one is accustomed to consider. It must always be noted whether the sky is clear or cloudy, whether it rains or snows. The extreme limits of the visual field for mixed light undergo such wide fluctuations that it is of little value to establish an average limit on the basis of a number of measurements. Changing illumination, conditioned by the time of day and of year during which the work is carried on, as well as the locality in which it is undertaken, produce variations in the same stimulus large enough to cause differences of from  $10^{\circ}$  to  $20^{\circ}$  [in the limit of sensitivity]. Especially in the nasal parts of the retina does the illumination influence the color limits, while their position remains more constant in the temporal retina." Fernald,<sup>2</sup> however, did make some attempt to obtain a standard illumination. She arranged white curtains at the windows of her optics-room, which could be lowered on bright days and drawn on dark days. This rather crude method was used also by Thompson and Gordon.<sup>3</sup> It is scarcely necessary to point out that the method lacks the first essential of standardization, namely, a means of measuring.

It is surprising that Wolffberg, as the logical corollary of his work, did not draw attention to the importance of standard-

<sup>1</sup> Ole Bull, 'Perimetric,' Bonn, 1895, p. 8.

<sup>2</sup> Fernald, G. M., 'The Effect of the Brightness of Background on the Extent of the Color Fields and on the Color Tone in Peripheral Retina,' *PSYCHOL. REV.*, 1905, XII., p. 392.

<sup>3</sup> Thompson and Gordon, 'A Study of After-images on the Peripheral Retina,' *PSYCHOL. REV.*, 1907, XIV., p. 122.

izing the illumination of the visual field in all work on the color sensitivity of the retina, and show how it could be accomplished by a modification of his method of working. He already had at hand one of the essentials for standardizing, namely, a method of changing the illumination of his room. The other essential, a method of measurement by means of which an illumination could be identified with a previous illumination chosen as standard, might have been derived from his results. For example, it would seem to have been a simple matter for him to have chosen as standard the particular illumination at which the red stimulus of 2.5 mm. diameter, the blue and green of 12 mm. each, the yellow of 4.5 mm., and the white of 2 mm. were just recognizable at a distance of 5 m. Stimuli of these sizes, it will be seen from the tables, were just recognizable at this distance at the illumination called 12/15, when 15/15 represents the illumination "bei günstige Tagesbeleuchtung." Using this condition as an index of the standard illumination, he could at any time have adjusted the illumination of the room by adding to or subtracting from the layers of tissue-paper covering the window, until the stimuli of these sizes were again just recognizable at the given distance. The accuracy and sensitivity of this method could have been tested by comparing the results of a series of determinations. An accurate and highly sensitive method sustaining some similarity in principle to the method suggested here is described in another paper in this volume of the REVIEW.<sup>1</sup>

### III. EXPERIMENTAL

The effect of change of illumination was forced upon our attention early in the investigation of the factors that influence the color sensitivity of the retina. For example, in preliminary work done by the writer on a well-lighted porch on Long Island, changes in color-tone were observed, when certain colors were compared in the central and in the peripheral retina, that are not found at all under the more intensive illumination of our optics-room when neither of the curtains is

<sup>1</sup> See footnote 2, p. 1.



drawn; and the peripheral limits of color were narrower by  $5^{\circ}$  to  $12^{\circ}$ . Furthermore, on a dark day, it was found that the limits of stimuli exposed through an opening in a white screen were reduced by about  $4^{\circ}$  as compared with the limits taken on a bright day. The change was less considerable with black and gray screens. The change in color-tone was most conspicuous in case of green.<sup>1</sup> On dark days, the green stimulus appeared as a pale unsaturated blue before becoming colorless in passing from the center to the periphery of the retina. This zone of blue was from  $7^{\circ}$  to  $23^{\circ}$  wide, in different meridians of the retina, with both white and black screens, but was wider with the black than with the white screen. On a sunny day, on the other hand, with the white screen green passed into bluish-green, then directly into gray, except in case of the upper regions where it appeared blue throughout a zone of about  $4^{\circ}$  in width. With the black screen, the blue zone was found only in the upper and temporal regions of the retina. The transition of green to yellow in the periphery that is generally reported in the literature was found in these experiments only when the gray screen was used. Yellow showed a color change that varied in amount with the degree of the general illumination. On a bright day with the white screen, it appeared reddish-orange. On a cloudy day, it was seen in the extreme periphery as a dark saturated red.

Working in our optics-room we found also that results taken on one day could not at all be duplicated on the following day. When the work was carried on under the most favorable conditions without special means of controlling illumination, namely, on bright days only, differences of  $5^{\circ}$  or more were found when the white screen was used. This necessitated a long series of observations if legitimate averages were to be obtained. Such a procedure is at best a poor makeshift and is besides of great disadvantage in many problems that come up in the work on color sensitivity. Particular instances of this may be found in investigations in which it is required to work in the region lying just within the limits of sensitivity, and in work on the after-images of stimuli in which no color is sensed.

<sup>1</sup> The green of the Hering series was used.

In the latter case the experiment requires that the stimulus be exposed just outside the limits of sensitivity determined with a given brightness condition, and that the observer should not be aware of the nature of the stimulus. In order to fulfill these requirements the experimenter must know the limits obtaining with a given brightness condition. It would be impossible to know this when the brightness conditions were subjected to the influence of changing illumination unless re-determinations were made at the beginning of each sitting and even frequently during its course. This would consume a great deal of time and would, besides, only roughly fulfill the requirements of the problem. A further and still more important example of the disadvantage may be found in the task we had set ourselves, namely, to investigate from point to point the sensitivity of the retina to each of the principal colors for three backgrounds in at least sixteen different meridians. In this work it is obvious that unless a standard illumination were provided, all comparative work would have to be done at one sitting. This is impossible. When time is taken between observations to guard against fatigue, at least three hours is required merely to outline the limits of sensitivity for a given color with one background for only one half of the retina. Even for this length of time there is no guarantee that the illumination has not altered. Thus at the outset of any extended investigation of color sensitivity, it is evident that, without a standard illumination, results will be of little comparative value.

In order better to know our factor and the ways in which it operates, a systematic investigation of the influence of changes of general illumination was carried on in our optics-room which is especially constructed to secure fine changes in illumination.<sup>1</sup> The experimentation was conducted by means of a rotary campimeter, described in full by Ferree, in the July number of the *American Journal of Psychology*.<sup>2</sup> Three observers acted as subjects. Since the results of all three are

<sup>1</sup> A description of this optics-room was given in the *PSYCHOLOGICAL REVIEW*, September number, 1912, pp. 367-368.

<sup>2</sup> Ferree, C. E., 'Description of a Rotary Campimeter,' *Amer. Journ. of Psychol.*, 1912, XXIII., pp. 449-453.

similar in their general bearing on the problem, space will be taken for the results of two of them only, *A* and *C*.

Rough preliminary experiments showed that the primary effect of decreasing the illumination was an increase in the amount of contrast induced across the stimulus by the campimeter screen. With the white screen, the increased induction was the most pronounced and was sufficient to cause large changes in the limits and in the color-tone of the stimulus. In order to investigate this effect in detail, gradual changes of illumination covering a wide range were made by means of the curtains with which our optics-room is furnished. Attention was given to the following points. (1) A quantitative estimate was made of the influence of change of illumination upon the brightness induction of the campimeter screen. (2) The effect of this induction upon the limits of color sensitivity was determined. (3) The limens of the colors were measured at different degrees of excentricity at different illuminations. And (4) the influence of change of illumination upon the effect of the preëxposure on the limens and limits of color was investigated. The degrees of illumination chosen for comparison were the standard illumination, the method of obtaining which was described in an earlier paper,<sup>1</sup> and a decreased illumination which was similar to that obtaining on a cloudy afternoon. Measured in foot-candles by means of the Sharpe-Millar portable photometer, the standard illumination equalled 390 foot-candles, the decreased 1.65 foot-candles.

#### I. QUANTITATIVE ESTIMATE OF THE INFLUENCE OF CHANGE OF ILLUMINATION UPON THE INDUCTION OF BRIGHTNESS BY THE SURROUNDING FIELD

The purpose of this investigation was to find out (1) how much the induction from white and black screens<sup>2</sup> is affected by a change in the general illumination; and (2) how much induction is gotten at decreased illumination from the gray screen which matches the color at standard illumination. The

<sup>1</sup> Ferree and Rand, *op. cit.*

<sup>2</sup> White and black screens are chosen because they represent the extreme cases of the effect of change of illumination.



induction in this latter case is caused by the change in the brightness relation between color and screens with decrease of illumination.<sup>1</sup> The campimeter screens served as inducing surface, grays of the brightness of the four principal colors of the Hering series both at standard and decreased illumination were used in turn as stimuli, and the amount of induction was estimated upon a measuring-disc, made up of adjustable sectors of the gray of the stimulus and white or black, according to the screen used. The measuring-disc was mounted on a motor which could be moved along the graded arm of the campimeter to any position from  $20^{\circ}$  to  $92^{\circ}$ . The gray stimulus was exposed through the opening of the screen in the usual manner. Two preliminary precautions were observed. (a) Since the brightness of the gray stimulus plus the induction of the screen was to be estimated by means of the measuring-disc, and since the brightness-value of the stimulus and of the disc changes with the amount of light that falls upon them, it was necessary to make sure before each measurement that the same amount of light fell upon each. This precaution was all the more necessary because the stimulus had to be placed behind the screen and the measuring-disc in front. In a given position of the apparatus, one or the other was apt to be shaded. The determination was made as follows: Measuring-disc, campimeter screen, and gray stimulus were all given the same brightness-value according to determinations made under conditions about which no doubt of the equality of the illumination of each could be entertained. Each was then placed in position for the experiment, and the position of the campimeter as a whole and of its various parts was adjusted until stimulus, screen, and measuring-disc were exactly matched in brightness-value. When an exact match was obtained we were guaranteed

<sup>1</sup> This latter determination is made to show that it is impossible to standardize the brightness of the surrounding field against the sudden and progressive changes of daylight that occur during the course of a single series of observations. These changes alter the brightness relation between the colored stimulus and the gray used as screen; therefore a match made at the beginning of a series will not hold throughout its course. For the same reason and to an equal degree the brightness relation between preëxposure and colored stimulus changes with change of illumination. It is, therefore, equally impossible to standardize the brightness of the preëxposure without some means of securing a standard illumination.

that all three were again equally illuminated. (b) The question arose whether brightness induction comes to its maximal value at once in the peripheral retina. A determination of the intensity curve of the contrast sensation was accordingly made at various points in the peripheral retina. It showed that contrast increases strongly for the first few seconds of stimulation. For this reason it was found to be necessary to make the judgment concerning the amount of induction of the screen, just as long after the induction had commenced as was done in the experiments to determine color sensitivity. In the color experiments an interval has to be allowed before the stimulus is exposed during which the observer obtains a steady fixation. During this interval of preëxposure, the eye is being stimulated by the campimeter screen and by the card which covers the stimulus. To prevent the preëxposure card from giving a brightness after-image which would fuse with and modify the stimulus, it should be chosen of a gray of the brightness of the color. In the same way, an interval has to be given in which to secure steady fixation when the amount of brightness induction is being measured. In order, then, to have the judgments made in each case the same length of time after induction had begun, it was necessary only to make the intervals of preëxposure of equal duration and to require that the judgments of each kind be made directly at the end of the preëxposure. In the case of the color experiments, the signal for the making of the judgment is the withdrawal of the preëxposure card and the exposure of the stimulus. For the judgments of induction, however, in which case the stimulus was the gray of the brightness of the color, it is obvious that no preëxposure card was needed, for preëxposure and stimulus were required by the conditions of the experiment to be the same. In this case, a word-signal had to be given to indicate the termination of the preëxposure interval and the instant at which the judgment was to be made.

*Results when White and Black Screens were Used.*—Observing these precautions as to the equality of the illumination of stimulus, screen, and measuring-disc, and as to the length of time the induction had had in which to increase before the

judgment was made, measurements were taken of the induction by white and black screens across grays of the brightness of the four principal colors at the illumination used. These measurements were made at various points of excentricity on the retina and for both standard and decreased illumination. The determination of the equality point between the stimulus and the measuring-disc was made as follows: The size of the white or black sector of the latter was changed until a preliminary judgment of equality was made. Then the j. n. d. on either side of this point was determined both by ascending and by descending series and an average of the results was taken as the final value of the induction. Measurements were taken at  $25^\circ$  and  $40^\circ$  on the temporal meridian, and at  $55^\circ$  and  $70^\circ$  on the nasal. The conditions at the nasal  $55^\circ$  point were very similar to those at  $25^\circ$  on the temporal side. The measurements at  $70^\circ$  nasal were midway in value between those at  $25^\circ$  and  $40^\circ$  on the temporal. The  $40^\circ$  point is very near the limits of color sensitivity in this meridian, and the induction here is very great. For one observer, the darker stimuli appeared black at this point, when the white background was used. In such cases, the difference between the induction at standard and at decreased illumination is more clearly shown by the observations made at  $25^\circ$  temporal meridian and at  $55^\circ$  and  $70^\circ$  nasal meridian than at  $40^\circ$  temporal. We have, however, chosen for two reasons to present in the following table only the results obtained in the temporal meridian. (1) The results obtained in this meridian demonstrate sufficiently well all the facts that need be taken into consideration. Space will not, therefore, be given to the results for both meridians. (2) The second point of our problem requires us to correlate the increased amount of induction caused by a given decrease of illumination with the change in the color limits it produces. The limits of color sensitivity can be more easily investigated in the temporal meridian because the sensitivity to some colors extends in the nasal region beyond the  $92^\circ$  point, which is the limit of measurement for the apparatus we used. This is true in particular in case of observer *C* as may be seen in Table XI. Both purposes of the investigation are, then,



better satisfied by results obtained in the temporal meridian.

The results show in general the following facts.

1. The amount of induction from the white screen is greater than that from the black screen.

2. The amount of induction increases with the distance from the fovea.

3. The amount of induction increases with decrease of illumination.<sup>1</sup>

4. The amount of increase under decreased illumination is greater in case of the white screen than in case of the black screen.

5. The white and black screens induce more contrast across the stimuli that are farthest removed from them in brightness, and least across those which are most like them. That is, the white screen induces more black across the gray of the brightness of blue than across a gray of the brightness of yellow; and the black screen induces more white across the gray of the brightness of yellow than across a gray of the brightness of blue.

Results are given in detail in Tables I. and II. Table I. gives the results for observer *A* taken on the temporal meridian, and Table II., the results for observer *C* for the same meridian. There is some difference in the amount of induction reported by the different observers, but since the preceding general statement of results is clearly borne out in every case, it is not deemed necessary to give space to results from all the observers used. In these tables, column 1 gives the degree of eccentricity at which the observation was made; columns 2, 3, and 4 show respectively the stimulus used, and the amounts of induction from the white and the black screens at standard illumination. Columns 5, 6, and 7 give the same data for decreased illumination.

*Results when the Gray Screen Matching the Colored Stimulus in Brightness at Standard Illumination is Used.*—It was neces-

<sup>1</sup> This statement is meant to apply only to the range of illumination worked with. The induction was not measured when the illumination was very low, nor when it was very intensive.

TABLE I

A. SHOWING THE AMOUNT OF CONTRAST INDUCED BY THE WHITE AND THE BLACK SCREENS AT STANDARD AND DECREASED ILLUMINATION UPON THE GRAYS OF THE BRIGHTNESSES OF THE COLORED STIMULI AT STANDARD AND DECREASED ILLUMINATION<sup>1</sup>

Fixation	Standard Illumination			Decreased Illumination		
	Stimulus (Gray of Brightness of Each of the Four Colors at Standard Illumination)	Amt. Induction of White Screen	Amt. Induction of Black Screen	Stimulus (Gray of Brightness of Each of the Four Colors at Decreased Illumination)	Amt. Induction of White Screen	Amt. Induction of Black Screen
25°	gray No. 2	Black 135°	White 110°	gray No. 2	Black 220°	White 170°
	gray No. 8	Black 155°	White 60°	gray No. 6	Black 270°	White 80°
	gray No. 24	Black 230°	White 28°	gray No. 41	Black 323°	White 40°
	gray No. 32	Black 290°	White 12°	gray No. 20	Black 330°	White 30°
40°	gray No. 2	Black 200°	White 300°	gray No. 3	Black 320°	White 360°
	gray No. 8	Black 300°	White 132°	gray No. 5	Black 360°	White 180°
	gray No. 24	Black 0°	White 60°	gray No. 50	Black 360° <sup>2</sup>	White 0° <sup>3</sup>
	gray No. 29	Black 360°	White 28°	gray No. 13	Black 360°	White 100°

<sup>1</sup> It is obvious that the method of expressing the amount of brightness induction used in this and the following tables gives an under-estimation. Suppose, as is shown in Table I., that No. 24 Hering gray has been darkened by induction until it matches in brightness a disc made up of 230° of black and 130° of the No. 24 gray. The amount of induction is greater than is represented by the 230° of black because the induction has not lessened the amount of light coming to the eye from the gray paper while the addition of 230° of black to the measuring-disc has cut off approximately 2/3 of the light coming from the gray paper. That is, in the one case enough black has been added by induction to reduce 360° of No. 24 gray to the given point in the brightness scale, while in the other enough black was added by direct mixing to lower only 130° of No. 24 gray to this point in the scale. Moreover, the underestimation will be increased by this method of measuring in proportion as the amount of induction is increased because the greater the induction is the more black and the less gray will have to be used in the measuring-disc. All that can be said accurately is that a certain gray darkened or lightened by induction matches in brightness a gray made up of a certain amount of the given gray plus a certain amount of black or white. The exact amount of the induction can not be separated out. Further, just because the brightness added by contrast does not alter the amount of light coming to the eye while the brightness added in any method of measurement does change this amount of light, the writer knows of no way by which an exact expression can be attained. The

TABLE II  
OBSERVER C

Fixation	Standard Illumination			Decreased Illumination		
	Stimulus (Gray of Brightness of Each of the Four Colors at Standard Illumination)	Amt. Induction of White Screen	Amt. Induction of Black Screen	Stimulus (Gray of Brightness of Each of the Four Colors at Decreased Illumination)	Amt. Induction of White Screen	Amt. Induction of Black Screen
25°	gray No. 2 gray No. 8 gray No. 24 gray No. 32	Black 70° Black 84° Black 93° Black 160°	White 55° White 48° White 30° White 15°	gray No. 2 gray No. 6 gray No. 40 gray No. 17	Black 130° Black 155° Black 187° Black 244°	White 70° White 59° White 45° White 22°
40°	gray No. 2 gray No. 7 gray No. 24 gray No. 27	Black 110° Black 142° Black 180° Black 214°	White 200° White 160° White 95° White 35°	gray No. 3 gray No. 4 gray No. 50 gray No. 7	Black 216° Black 230° Black 360° <sup>4</sup> Black 300°	White 340° White 320° White 0° <sup>5</sup> White 108°

method she has used, however, does serve as a means of comparing the amounts of induction occurring under different conditions sufficiently accurately for her purpose at this point.

<sup>2</sup>The gray No. 50 was in reality rendered blacker by the inductive action of gray No. 24 than the Hering black we used on the measuring-disc. A match thus could not be attained with black 360° as the table indicates.

<sup>3</sup>There was no brightness induction in this case because the stimulus, gray No. 50, matches in brightness the black paper which formed the campimeter screen.

<sup>4</sup>See footnote 2 above.

<sup>5</sup>See footnote 3 above.



sary to perform the experiments bearing on this point at decreased illumination only. For them the campimeter screens which matched in brightness the four principal colors of the Hering series at standard illumination served as inducing surfaces. For the contrast surfaces, grays of the brightness of these colors at decreased illumination were chosen. The methods of measuring, precautions in working, parts of the retina investigated, etc., were the same as in the preceding determinations. The following general statement of results may be made. (1) At the  $25^{\circ}$  point the brightness of yellow was found not to have changed at all with the decrease of illumination produced by changing the illumination from the value selected as standard to the value selected for the comparison; the brightness of green lightened by an amount equal to the difference between No. 8 and No. 6 of the Hering series of grays; red darkened by an amount equal to the difference between No. 24 and No. 40; and blue lightened by an amount equal to the difference between No. 32 and No. 20. The amount of induction by the gray screen of the original brightness of the color upon the gray stimulus of the brightness of the color as altered by the decreased illumination, expressed in terms of Hering white and black, was for yellow  $0^{\circ}$ , for green  $60^{\circ}$  of white, for red  $27^{\circ}$  of black, and for blue  $20^{\circ}$  of white. (2) At the  $40^{\circ}$  point, the yellow darkened by an amount equal to the difference between No. 2 and No. 3 of the Hering grays; green lightened by an amount equal to the difference between No. 8 and No. 5; red darkened by an amount equal to the difference between No. 28 and No. 50; and blue lightened by an amount equal to the difference between No. 28 and No. 13. The amount of induction produced by these changes was for yellow  $280^{\circ}$  of black, for green  $130^{\circ}$  of white, for red  $360^{\circ}$  of black, and for blue  $60^{\circ}$  of white. These results are shown in detail in Table III.

## 2. THE EFFECT OF THESE AMOUNTS OF INDUCTION UPON THE LIMITS OF COLOR SENSITIVITY

In order to obtain an estimate of the range of effect upon the limits of color sensitivity of the induction of the screens

TABLE III

A. SHOWING THE AMOUNT OF CONTRAST INDUCED AT DECREASED ILLUMINATION ON GRAYS OF THE BRIGHTNESS OF THE COLORS AT DECREASED ILLUMINATION BY THE GRAY SCREENS MATCHING THE COLORS IN BRIGHTNESS AT STANDARD ILLUMINATION

Fixation	Stimulus	Screen	Amount of Induction
25°	gray No. 2	gray No. 2	0°
	gray No. 6	gray No. 8	white 60°
	gray No. 41	gray No. 24	black 27°
	gray No. 20	gray No. 32	white 20°
40°	gray No. 3	gray No. 2	black 280°
	gray No. 5	gray No. 8	white 130°
	gray No. 50	gray No. 24	black 360° <sup>1</sup>
	gray No. 13	gray No. 28	white 60°

at standard and at decreased illumination, the breadth of the color zones was determined at both illuminations (1) when white and black served in turn as campimeter screens; and (2) when a gray matching the color in brightness at standard illumination was used. The preëxposure was in each case to a gray of the same brightness as the stimulus at the illumination used. The point at which the color lost all trace of its original quality was recorded as the limit of sensitivity.

*Results when White and Black Screens Were Used.*—When the stimulus color is gotten by reflection from a pigment surface, two factors operate to give a change of result when the illumination is decreased. (1) There is a decrease in the amount of colored light coming to the eye. (2) There is an increase in the inductive action of the screen due to the change in the brightness relation of stimulus to screen and to the increased sensitivity of the eye to brightness contrast at decreased illumination.

In order to find out how much of our results with the white and black screens should be attributed to the decrease in the amount of colored light coming to the eye produced by the decreased illumination, and how much to the increased inductive action of the screens, the limits of sensitivity were also determined at both illuminations with the screens of the gray into

<sup>1</sup> The gray No. 50 was in reality rendered blacker by the inductive action of gray No. 24 than the Hering black we used on the measuring-disc. A match thus could not be attained with black 360° as the table indicates.

which the color disappears in the peripheral retina. From the values obtained with the three screens at both illuminations, the amount of change due to decrease in the amount of colored light coming to the eye and the amount due to induction by the white and black screens were calculated as follows. (a) From the number of degrees expressing the limits for a given color at standard illumination with a screen of the brightness of the color at that illumination was subtracted the number expressing its limit at decreased illumination with a screen of the brightness of the color at the decreased illumination. That this gave the number of degrees the zone of sensitivity was narrowed by the decrease in the energy of the stimulus may be said with the following qualification. If there is any influence upon color sensitivity of the local brightness-adaptation of the retina produced by the change in the general illumination, it is, of course, included in this effect. But, since this influence would have to be brought about by previous exposure to the illumination in question, it can be reduced to a minimum by guarding against an exposure to it for any considerable length of time. The effect of whatever adaptation there may be, however, can not be isolated or separated out from the above result, and the value expressing the amount the limit is narrowed by the actual decrease of the energy of colored light coming to the eye cannot, strictly speaking, be obtained. But it is probable that the adaptation effect is not sufficiently strong to influence the limits, since the sensitivity of the extreme peripheral retina falls off very abruptly from point to point. The difference, then, between the color limit obtained at standard illumination and the limit at decreased illumination, when in both cases there is no brightness induction from the screen, may be said to approximate the effect upon the limits produced by the decrease in the amount of colored light coming to the eye. (b) Figures can be obtained, however, from our results, which express the amount by which the zones are narrowed by the change in the inductive action of the white and black screens produced by decreasing the illumination, that are not open to theoretical questioning; for the influence of local brightness-adaptation, if there be any, is a constant for all screens at the same illu-



mination. If then, the number of degrees which expresses the limits of sensitivity for either the white or the black screen at decreased illumination is subtracted from the number expressing the limit with a screen of the gray of the brightness of the color at this illumination, the result will represent the extent to which the limit was narrowed by the action of induction alone. The results show in general the following facts:

1. At standard illumination, induction from the white screen narrows the limits of yellow and red; induction from the black screen narrows the limits of blue and green. The difference is in no case more than  $4^{\circ}$ .

2. At decreased illumination, the induction from the white screen narrows the limits of all the colors much more considerably than does the induction from the black screen.<sup>1</sup>

3. The values expressing the narrowing of the limits caused by decrease of illumination without induction are greatest in case of those colors which undergo maximum change of brightness in passing into the periphery, namely, for blue and red.

We have shown by the results of the preceding section, that the increased induction produced by decrease of the general illumination is greater for the white screen than for the black, and, by the results of this section, that this increase is effective to the extent of narrowing the limits of sensitivity to all colors from  $5^{\circ}$  to  $12^{\circ}$  with this screen. With the black screen, the limits were narrowed from  $3^{\circ}$  to  $6^{\circ}$ . At standard illumination, the limits were narrowed only from  $1^{\circ}$  to  $4^{\circ}$  with either the white or the black screens.

Results in detail are given in Tables IV. and V., taken from the temporal meridians of the observers whose observations are recorded in Tables I. and II. In column 1, Tables IV. and V., is given the stimulus. Column 2 shows the limit of sensitivity to the stimulus at standard illumination with a screen of a

<sup>1</sup> For observer *A* the results for green present an exception. At the decreased illumination used the green stimulus appeared bluish in the central retina. The induction of the black screen caused it to appear as a pale blue at a comparatively slight degree of excentricity. According to our definition of color limit, this point is the limit of green. It is, however, obvious that the exception is due rather to the qualitative than to the quantitative effect of brightness upon color.

gray of the brightness of the color at standard illumination; column 3 shows the limit with a white screen; and column 4 with a black screen. Column 5 shows the limit at decreased illumination with a screen of the brightness of the color at decreased illumination; column 6 shows the limit with a white screen; and column 7 with a black screen.

TABLE IV

A. SHOWING THE COLOR LIMITS AT STANDARD AND DECREASED ILLUMINATION (a) WITH GRAY SCREENS OF THE BRIGHTNESSES OF THE COLORS AT THE ILLUMINATION USED; AND (b) WITH WHITE AND BLACK SCREENS

Stimulus	Standard Illumination			Decreased Illumination		
	Limit with Gray Screen of Brightness of Color at Standard Illumination	Limit with White Screen	Limit with Black Screen	Limit with Gray Screen of Brightness of Color at Decreased Illumination	Limit with White Screen	Limit with Black Screen
Yellow.....	44°	42°	45°	43°	35°	43°
Green.....	37°	37°	34°	36°	31°	27°
Red.....	43°	42°	44°	40°	31°	40°
Blue.....	50°	50°	49°	48°	36°	43°

TABLE V

OBSERVER C

Stimulus	Standard Illumination			Decreased Illumination		
	Limit with Gray Screen of Brightness of Color at Standard Illumination	Limit with White Screen	Limit with Black Screen	Limit with Gray Screen of Brightness of Color at Decreased Illumination	Limit with White Screen	Limit with Black Screen
Yellow.....	49°	46°	50°	46°	36°	44°
Green.....	44°	42°	40°	41°	28°	33°
Red.....	45°	41°	45°	41°	34°	41°
Blue.....	56°	55°	53°	50°	38°	44°

Tables VI. and VII. have been compiled from Tables IV. and V. to show the following facts.

(1) How much the decrease of illumination narrowed the limits of color sensitivity by causing a decrease in the energy of the light waves coming to the eye. This was determined by subtracting the value of the limit at decreased illumination with the screen of a gray of the brightness of the color at decreased illumination from its value at full illumination with the gray screen of the brightness of the color at full illumination.

(2) How much the limits were narrowed by the action of the white and black screens at decreased illumination. This was ascertained by subtracting the values of the limit with the white and the black screen at decreased illumination from the value of the limit at decreased illumination with the gray screen of the brightness of the color at this illumination. (3) How much more the limits were narrowed by the white and the black screens at decreased than at full illumination. This was computed for the white screen, for example, as follows. The quantity, limit at decreased illumination for gray screen of brightness of color at decreased illumination, minus limit for white screen at decreased illumination, is subtracted from the

TABLE VI

A. SHOWING (1) HOW MUCH THE LIMITS WERE NARROWED BY DECREASE IN THE AMOUNT OF COLORED LIGHT COMING TO THE EYE; (2) HOW MUCH THEY WERE NARROWED BY INCREASED INDUCTION OF WHITE AND BLACK SCREENS AT DECREASED ILLUMINATION; AND (3) HOW MUCH MORE THEY WERE NARROWED BY INDUCTION OF WHITE AND BLACK SCREENS AT DECREASED THAN AT FULL ILLUMINATION

Stimulus	How Much Limits were Narrowed by Decrease in Amount of Colored Light Coming to the Eye	How Much Limits were Narrowed by Induction of White Screen	How Much Limits were Narrowed by Induction of Black Screen	How Much More Limits were Narrowed by White Screen at Decreased than at Full Illumination	How Much More Limits were Narrowed by Black Screen at Decreased than at Full Illumination
Yellow. ....	1°	8°	0°	6°	3°
Green. ....	1°	5°	9°	5°	6°
Red. ....	3°	9°	0°	8°	3°
Blue. ....	2°	12°	5°	12°	4°

TABLE VII

OBSERVER C

Stimulus	How Much Limits were Narrowed by Decrease in Amount of Colored Light Coming to the Eye	How Much Limits were Narrowed by Induction of White Screen	How Much Limits were Narrowed by Induction of Black Screen	How Much More Limits were Narrowed by White Screen at Decreased than at Full Illumination	How Much More Limits were Narrowed by Black Screen at Decreased than at Full Illumination
Yellow. ....	3°	10°	2°	7°	3°
Green. ....	3°	13°	8°	11°	4°
Red. ....	4°	7°	0°	3°	0°
Blue. ....	6°	12°	7°	11°	3°



quantity, limit at full illumination for gray screen of brightness of color at full illumination minus limit for white screen at full illumination. A similar computation was made for the black screen.

*Results when a Gray Screen Matching the Color in Brightness at Standard Illumination is Used.*—In these experiments a determination was made of the amount the limits of sensitivity are changed by the brightness induction caused by the alteration of the brightness relation between stimulus and screen with decrease of illumination, when a screen is used which matches the color in brightness at standard illumination. This determination was made as follows. An estimate was made of the amount the limits were narrowed by decrease of illumination when a screen of the brightness of the color at standard illumination is used for both standard and decreased illuminations. From this result was subtracted the amount the limits were narrowed by decrease of illumination when the screen is made in turn of the brightness of the color at standard and decreased illumination. The difference obtained represents the value sought. It is given in Table VIII.

TABLE VIII

A. SHOWING HOW MUCH THE COLOR LIMITS WERE NARROWED AT DECREASED ILLUMINATION BY THE INDUCTION OF THE SCREEN WHICH MATCHED THE COLOR IN BRIGHTNESS AT STANDARD ILLUMINATION

Stimulus	Screen of Brightness of Color at Decreased Illumination	Limit	Screen of Brightness of Color at Standard Illumination	Limit	Amount Limit was Narrowed by Change in Brightness Relation Between Stimulus and Screen Caused by Decreased Illumination
Yellow...	gray No. 3	43°	gray No. 2	41°	2°
Green...	gray No. 5	36°	gray No. 8	29°	7°
Red.....	gray No. 50	40°	gray No. 24	33°	7°
Blue.....	gray No. 13	48°	gray No. 28	46°	2°

### 3. THE EFFECT OF THESE AMOUNTS OF INDUCTION UPON THE LIMEN OF COLOR AT DIFFERENT DEGREES OF EXCENTRICITY

We have shown the effect of decreasing the general illumination upon the color sensitivity of the peripheral retina with

gray, white, and black screens, by the effect on the limits of sensitivity. This is only an indirect means of estimating its influence, for the results obtained cannot be translated into terms of direct measurement, owing to the irregular decrease in sensitivity of the peripheral retina from the fovea outwards. In this section, we shall measure the influence of changes of illumination directly by the changes produced in the limen of sensation at various angles of excentricity. As in the previous section, measurement will be made of the effect upon sensitivity (1) of the decrease in the amount of colored light coming to the eye, produced by the decrease of illumination, (2) of the difference in the inducing power of the white and black screens, and (3) of the change in the brightness relation of stimulus to background.

To determine the first of these three points, a campimeter screen had to be selected that gave no brightness contrast with the stimulus. To provide for differences in the brightness of the colors at the different points observed for the two illuminations at which we worked, a preliminary determination of the brightness of the sensation at these points was made at both illuminations by the flicker method. The brightness of the screen was chosen in each case of the brightness of the color according to these determinations. To eliminate the effect of preëxposure, the stimulus previous to exposure was in every case covered by a gray of the brightness of the color for the illumination used at the point of the retina at which we were working. Thus no brightness after-image was carried over to exert an inhibitive action upon the color sensation. The stimulus was a disc compounded of sectors of the color and of the gray of the brightness of the color for the illumination used at the point of the retina under investigation. The proportions of the sectors were altered until the observer gave the judgment of just noticeable color. The average of judgments made in ascending and descending series was chosen as the final value of the limen. The difference between the limens at standard and decreased illumination was taken as the measure of the loss in intensity which the stimulus had sustained by the decrease of illumination.

The effect upon the color limen of the increased induction from the white and black screens was shown by the same method, with the exception that the white and black screens were substituted for the gray of the brightness of the color. The stimulus was a disc composed of sectors of color and gray of the brightness of the color at the angle of excentricity at which the determination was made.

The effect of the change in the brightness relation between the stimulus color and the screen produced by decrease of illumination was shown as follows. An estimate was made of the amount the limens are raised by the decrease of illumination when a screen was used for both standard and decreased illumination that had a brightness value equal to the color at standard illumination. From these results was subtracted the amount the limens were raised by decreasing the illumination when the screens were made in turn of the brightness of the color at standard and at decreased illumination. The difference obtained represents the value sought. These results are of particular importance because they show that the influence of the brightness of the surrounding field can not be eliminated even when a screen of the brightness of the color is used unless some means be had of maintaining the general illumination of the room constant.

Table IX. shows how much the limens of sensitivity were raised at the fovea and at points  $15^\circ$ ,  $25^\circ$  and  $30^\circ$  from the fovea in the horizontal meridian on the temporal side by the decrease in the amount of colored light coming to the eye produced by the decrease in the general illumination. The results of this table may be generalized as follows:

1. The limen of color is higher in the periphery than in the center of the retina at both illuminations.
2. The limen of color is higher at decreased illumination than at standard illumination.
3. The direct effect upon the intensity of the sensation produced by decreasing the illumination is shown by the limen determinations to be inconsiderable. In the central retina, the difference is but  $2^\circ$  or  $3^\circ$ . In the peripheral retina at the points considered there is a difference of from  $10^\circ$  to  $20^\circ$ .



TABLE IX

A. SHOWING HOW MUCH THE LIMENS OF SENSITIVITY WERE RAISED AT THE FOVEA, AND AT POINTS  $15^{\circ}$ ,  $25^{\circ}$ ,  $30^{\circ}$  FROM THE FOVEA IN THE HORIZONTAL MERIDIAN ON THE TEMPORAL SIDE BY THE DECREASE IN THE AMOUNT OF COLORED LIGHT COMING TO THE EYE PRODUCED BY THE DECREASE IN THE GENERAL ILLUMINATION

Stimulus	Point on Horizontal Temporal Meridian at Which Limen was Taken	Limen at Standard Illumination with Screen of Brightness of Color at Standard Illumination	Limen at Decreased Illumination with Screen of Brightness of Color at Decreased Illumination	How Much Limen was Raised at Decreased Illumination
Yellow.....	$0^{\circ}$ $15^{\circ}$ $25^{\circ}$ $30^{\circ}$	$18^{\circ}$ $22^{\circ}$ $35^{\circ}$ $50^{\circ}$	$20^{\circ}$ $32^{\circ}$ $40^{\circ}$ $65^{\circ}$	$2^{\circ}$ $10^{\circ}$ $5^{\circ}$ $15^{\circ}$
Green.....	$0^{\circ}$ $15^{\circ}$ $25^{\circ}$	$20^{\circ}$ $27^{\circ}$ $40^{\circ}$	$20^{\circ}$ $28^{\circ}$ $50^{\circ}$	$0^{\circ}$ $1^{\circ}$ $10^{\circ}$
Red.....	$0^{\circ}$ $15^{\circ}$ $25^{\circ}$ $30^{\circ}$	$9^{\circ}$ $9^{\circ}$ $17^{\circ}$ $25^{\circ}$	$11^{\circ}$ $13^{\circ}$ $25^{\circ}$ $45^{\circ}$	$2^{\circ}$ $4^{\circ}$ $8^{\circ}$ $20^{\circ}$
Blue.....	$0^{\circ}$ $15^{\circ}$ $25^{\circ}$ $30^{\circ}$	$9^{\circ}$ $10^{\circ}$ $12^{\circ}$ $20^{\circ}$	$10^{\circ}$ $13^{\circ}$ $15^{\circ}$ $40^{\circ}$	$1^{\circ}$ $3^{\circ}$ $3^{\circ}$ $20^{\circ}$

TABLE X

A. SHOWING THE COLOR LIMENS AT STANDARD AND DECREASED ILLUMINATIONS WITH WHITE AND WITH BLACK SCREENS

Stimulus	Point on Horizontal Temporal Meridian at Which Limen was Taken	White Screen		Black Screen	
		Limen at Standard Illumination	Limen at Decreased Illumination	Limen at Standard Illumination	Limen at Decreased Illumination
Yellow...	$0^{\circ}$ $15^{\circ}$ $25^{\circ}$ $30^{\circ}$	$22^{\circ}$ $25^{\circ}$ $50^{\circ}$ $80^{\circ}$	$25^{\circ}$ $50^{\circ}$ $80^{\circ}$ $125^{\circ}$	$28^{\circ}$ $35^{\circ}$ $42^{\circ}$ $60^{\circ}$	$30^{\circ}$ $45^{\circ}$ $60^{\circ}$ $90^{\circ}$
Green....	$0^{\circ}$ $15^{\circ}$ $25^{\circ}$	$22^{\circ}$ $26^{\circ}$ $30^{\circ}$	$25^{\circ}$ $36^{\circ}$ $75^{\circ}$	$28^{\circ}$ $35^{\circ}$ $75^{\circ}$	$30^{\circ}$ $43^{\circ}$ $220^{\circ}$
Red.....	$0^{\circ}$ $15^{\circ}$ $25^{\circ}$ $30^{\circ}$	$13^{\circ}$ $19^{\circ}$ $30^{\circ}$ $50^{\circ}$	$20^{\circ}$ $35^{\circ}$ $55^{\circ}$ $330^{\circ}$	$10^{\circ}$ $13^{\circ}$ $23^{\circ}$ $29^{\circ}$	$14^{\circ}$ $19^{\circ}$ $35^{\circ}$ $58^{\circ}$
Blue.....	$0^{\circ}$ $15^{\circ}$ $25^{\circ}$ $30^{\circ}$	$17^{\circ}$ $25^{\circ}$ $35^{\circ}$ $40^{\circ}$	$22^{\circ}$ $40^{\circ}$ $60^{\circ}$ $90^{\circ}$	$10^{\circ}$ $12^{\circ}$ $18^{\circ}$ $30^{\circ}$	$12^{\circ}$ $17^{\circ}$ $25^{\circ}$ $60^{\circ}$

Table X. shows the color limens at both standard and decreased illuminations when white and black screens are used, at the fovea and at points  $15^\circ$ ,  $25^\circ$ , and  $30^\circ$  in the horizontal meridian on the temporal side.

Table XI. has been compiled from Tables IX. and X. to show how much greater the limens were for white and black screens at decreased than at full illumination; how much of the effect may be ascribed to the reduction of the amount of colored light coming to the eye; and how much to the increased induction of the screens. It will be seen from the results of this table that the loss of the sensation in intensity due to the increased brightness induction is much greater than that caused by the reduction in the amount of colored light coming to the eye.

TABLE XI

A. SHOWING HOW MUCH GREATER THE LIMENS WERE WITH WHITE AND BLACK SCREENS AT DECREASED THAN AT STANDARD ILLUMINATION AND HOW MUCH OF THIS EFFECT MAY BE ASCRIBED TO THE REDUCTION IN THE AMOUNT OF COLORED LIGHT COMING TO THE EYE AND HOW MUCH TO THE INCREASED INDUCTIVE ACTION OF THE SCREENS

Stimulus	Point on Horizontal Temporal Meridian at Which Limen was Taken	White Screen			Black Screen		
		Total Amount Greater	Amount Due to Decrease in Amount of Colored Light Coming to Eye	Amount Due to Increased Induction of Screen	Total Amount Greater	Amount Due to Decrease in Amount of Colored Light Coming to Eye	Amount Due to Increased Induction of Screen
Yellow...	$0^\circ$	$7^\circ$	$2^\circ$	$5^\circ$	$12^\circ$	$2^\circ$	$10^\circ$
	$15^\circ$	$28^\circ$	$10^\circ$	$18^\circ$	$23^\circ$	$10^\circ$	$13^\circ$
	$25^\circ$	$45^\circ$	$5^\circ$	$40^\circ$	$25^\circ$	$5^\circ$	$20^\circ$
	$30^\circ$	$75^\circ$	$15^\circ$	$60^\circ$	$35^\circ$	$15^\circ$	$25^\circ$
Green...	$0^\circ$	$5^\circ$	$0^\circ$	$5^\circ$	$10^\circ$	$0^\circ$	$10^\circ$
	$15^\circ$	$8^\circ$	$1^\circ$	$7^\circ$	$16^\circ$	$1^\circ$	$15^\circ$
	$25^\circ$	$35^\circ$	$10^\circ$	$25^\circ$	$180^\circ$	$10^\circ$	$170^\circ$
Red.....	$0^\circ$	$11^\circ$	$2^\circ$	$9^\circ$	$5^\circ$	$2^\circ$	$3^\circ$
	$15^\circ$	$26^\circ$	$4^\circ$	$22^\circ$	$10^\circ$	$4^\circ$	$6^\circ$
	$25^\circ$	$38^\circ$	$8^\circ$	$30^\circ$	$18^\circ$	$8^\circ$	$10^\circ$
	$30^\circ$	$305^\circ$	$20^\circ$	$285^\circ$	$33^\circ$	$20^\circ$	$13^\circ$
Blue.....	$0^\circ$	$13^\circ$	$1^\circ$	$12^\circ$	$3^\circ$	$1^\circ$	$2^\circ$
	$15^\circ$	$30^\circ$	$3^\circ$	$27^\circ$	$7^\circ$	$3^\circ$	$4^\circ$
	$25^\circ$	$48^\circ$	$3^\circ$	$45^\circ$	$13^\circ$	$3^\circ$	$10^\circ$
	$30^\circ$	$70^\circ$	$20^\circ$	$50^\circ$	$40^\circ$	$20^\circ$	$20^\circ$

It was shown in Table III. that quite a great deal of brightness induction is caused by the change in brightness relation between color and screen produced by decreasing the illumination. Table VIII. shows how much this induction narrows the limits of sensitivity to the four colors used. Table XII. shows how much the limens are raised when the illumination is decreased by the inductive action caused by the change in the brightness relation between stimulus color and gray screen of the brightness of the color at standard illumination.

TABLE XII

A. SHOWING HOW MUCH THE COLOR LIMENS WERE RAISED AT DECREASED ILLUMINATION BY THE INDUCTION OF THE SCREENS WHICH MATCHED THE COLOR AT STANDARD ILLUMINATION

Stimulus	Point on Horizontal Temporal Meridian at Which Limen was Taken	Limen with Screen of Brightness of Color at Decreased Illumination	Limen with Screen of Brightness of Color at Standard Illumination	Amount Limen was Raised by Change in Brightness Relation Between Stimulus and Screen Caused by Decrease of Illumination
Yellow.....	0°	20°	20°	0°
	15°	32°	32°	0°
	25°	40°	40°	0°
	30°	65°	116°	51°
Green.....	0°	20°	20°	0°
	15°	28°	40°	12°
	25°	50°	190°	140°
Red.....	0°	11°	11°	0°
	15°	13°	24°	11°
	25°	25°	48°	23°
	30°	45°	150°	105°
Blue.....	0°	10°	12°	2°
	15°	13°	16°	3°
	25°	15°	23°	8°
	30°	40°	55°	15°

#### 4. THE INFLUENCE OF CHANGE OF ILLUMINATION UPON THE ACTION OF THE PREEXPOSURE ON THE LIMENS AND LIMITS OF COLOR

The brightness of the preexposure exerts an influence upon the color observation because the eye carries over an after-image from the preexposure into the color observation. If, for example, the preexposure is to black, a white after-image is



aroused which fuses with the succeeding color sensation and strongly reduces its saturation. The effect of preëxposure is especially strong in the peripheral retina because a very strong brightness after-image is aroused in the peripheral retina by a very short period of stimulation. It is very difficult for the writer to predict from the data she has at hand with regard to the effect of change of illumination upon the sensitivity of the peripheral retina to the brightness after-image just what will be the effect of change of illumination upon the action of preëxposure on the color sensitivity of the peripheral retina. But even though there be no change in the sensitivity of the peripheral retina to the brightness after-image with change of illumination, it is obvious that there will be some effect of the change of illumination because of the change in the brightness relation of the preëxposure card to the colored stimulus. In case the stimulus light is gotten by reflection from pigment surfaces, this change of brightness relation is due to the shift in the brightness of the colors produced by the change in the illumination. In case transmitted light is used as stimulus, the brightness of the stimulus color is independent of changes in illumination and will remain constant; but a change in the brightness relation of stimulus to preëxposure will occur because the preëxposure will lighten or darken with change of illumination. The writer hopes to make the quantitative investigation of this point the subject of a future study. At present she can only point out that if a guarantee is wanted that the effect of the brightness of the preëxposure is eliminated from the results of the observation, the preëxposure must be to a gray of the brightness of the color and the illumination of the room must be kept constant.

#### IV. CONCLUSION

The foregoing results show how strongly the changes in the illumination of the visual field influence the color sensitivity of the peripheral retina, particularly when the stimulus is surrounded by a white field. They also show that the influence of surrounding field can not be eliminated even by means of a campimeter screen of the brightness of the color unless some

means be had of keeping the general illumination of the room constant. It is obvious without further comment how important it is that a method be devised to standardize this factor. The preceding experiments indicate that without this standardization, no experiment can be repeated from time to time under the same conditions relative to any one of the brightness factors that influence color sensitivity. Results thus obtained are far from comparable. As was stated earlier in the paper a method of standardizing was described in an earlier paper in this volume of the *REVIEW*.<sup>1</sup>

<sup>1</sup>See footnote, 2, p. 1.

It was the writer's intention to have had the present article precede the one in which the method of standardizing is described, but owing to limited space in the July number the Editor was compelled to use for that number the shorter article.

# NOTE ON A RETRIAL OF PROFESSOR JAMES' EXPERIMENT ON MEMORY TRAINING

BY HARVEY A. PETERSON

*Illinois State Normal University*

The experiments referred to may be found on pages 666-7 of Vol. I. of 'The Principles of Psychology.' The material chosen for the training was the first and second books of Milton's 'Paradise Lost,' while for the tests Tennyson's 'The Coming of Arthur' and 'Guinevere' were used. The subjects were Mrs. Peterson and myself. As we learned different poems, each could hear the other's recital. In the preliminary test 16 lines of Tennyson were learned each day for six days, the passages being consecutive, barring one or two unsuitable passages. These were also the arrangements of the final test. The training consisted of the same amount of 'Paradise Lost' per day for 29 consecutive days. The time of learning was kept uniform. Seven normal school students, three boys and four girls, acted as control subjects, taking only the tests. The following table gives the results. The figures are minutes

TRAINING SUBJECTS

	Training Averages			Test Averages		
	Prelim.	Final	Gain, Per Cent.	Prelim.	Final	Gain, Per Cent.
H.A.P.	27.26	20.61	24.4	20.3	13.6	33
M.H.P.	17.30	20.37	-17.7	18.5	10.6	42.7

CONTROL SUBJECTS

'The Coming of Arthur'				'Guinevere'			
Subj.	Prelim.	Final	Gain, Per Cent.	Subj.	Prelim.	Final	Gain, Per Cent.
E.R.	15.73	10.31	34.5	D.B.	16.85	20.63	-22.4
B.H.	12.35	9.46	23	G.K.	16.02	16.66	-4
H.F.	24.74	17.51	29.2				
C.J.	35.53	24.04	32.3				
L.W.	25.06	21.12	15.3				
Average Improvement 26.8				Average Improvement -13.2			



and fractions of minutes. Where percentages are given, they are percentages of improvement.

In calculating the improvement in training, averages of the first six and the last six days were compared.

The large gains made by all except one of the control subjects who learned 'The Coming of Arthur' was due partly to the fact that the part learned in the final test was not so difficult as that learned in the preliminary test.

After the average improvement of the control subjects has been subtracted, H.A.P. made a net improvement of 6.2 per cent. in the tests, which is about one fourth of the improvement made by him in training. M.H.P. made an improvement of about 56 per cent. in the tests by a similar calculation. In the training no lasting improvement was made. Nevertheless there was certainly a large transfer of training here.

The improvement of both training subjects is ascribed to getting practice in methods of verbatim memorizing commonly considered by psychologists to be the best, such as learning by reading the sixteen lines over as a whole time after time rather than learning piecemeal, reading rapidly after the sense has been grasped, skeletonizing the thought, and others.











BF

1

P7

v.19

Psychological review

For use in  
the Library  
ONLY

PLEASE DO NOT REMOVE  
CARDS OR SLIPS FROM THIS POCKET

---

UNIVERSITY OF TORONTO LIBRARY

---



